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UPON THE PRODUCTION OF SOUND BY RADIANT ENERGY.*

By Alexander Graham Bell.

In a paper read before the American Association for the Advancement of Science, last August, I described certain experiments made by Mr. Sumner Tainter and myself which had resulted in the construction of a "*Photophone*," or apparatus for the production of sound by light;† and it will be my object to-day to describe the progress we have made in the investigation of photophonic phenomena since the date of this communication.

In my Boston paper the discovery was announced that thin disks of very many different substances *emitted sounds* when exposed to the action of a rapidly-interrupted beam of sunlight. The great variety of material used in these experiments led me to believe that sonorousness under such circumstances would be found to be a general property of all matter.

At that time we had failed to obtain audible effects from masses of the various substances which became sonorous in the condition of thin diaphragms, but this failure was explained upon the supposition that the molecular disturbance produced by the light was chiefly a surface action, and that under the circumstances of the experiments the vibration had to be transmitted through the mass of the substance in order to affect the ear. It was, therefore, supposed that, if we could lead to the ear air that was directly in contact with the illuminated surface, louder sounds might be obtained, and solid masses be found to be as sonorous as thin diaphragms. The first experiments made to verify this hypothesis pointed toward success. A beam of sunlight was focused into one end of an open tube, the ear being placed at the other end. Upon interrupting the beam, a clear, musical tone was heard, the pitch of which depended upon the frequency of the interruption of the light and the loudness upon the material composing the tube.

At this stage our experiments were interrupted, as circumstances called me to Europe.

While in Paris a new form of the experiment occurred to my mind, which would not only enable us to investigate the sounds produced by masses, but would also permit us to test the more general proposition that *sonorousness, under the influence of intermittent light, is a property common to all matter.*

The substance to be tested was to be placed in the interior of a transparent vessel, made of some material which (like glass) is transparent to light, but practically opaque to sound.

Under such circumstances the light could get in, but the sound produced by the vibration of the substance could not get out. The audible effects could be studied by placing the ear in communication with the interior of the vessel by means of a hearing tube.

Some preliminary experiments were made in Paris to test this idea, and the results were so promising that they were communicated to the French Academy on the 11th of October, 1880, in a note read for me by Mr. Antoine Breguet.* Shortly afterwards I wrote to Mr. Tainter, suggesting that he should carry on the investigation in America, as circumstances prevented me from doing so myself in Europe. As these experiments seem to have formed the common starting point for a series of independent researches of the most important character, carried on simultaneously, in America by Mr. Tainter, and in Europe by M. Mercadier,* Prof. Tyndall,† W. E. Röntgen,‡ and W. H. Preece,§ I may be permitted to quote from my letter to Mr. Tainter the passage describing the experiments referred to:

"Metropolitan Hotel, Rue Cambon, Paris, Nov. 2, 1880.

"Dear Mr. Tainter:

. . . "I have devised a method of producing sounds by the action of an intermittent beam of light from substances that cannot be obtained in the shape of thin diaphragms or in the tubular form; indeed, the method is specially adapted to testing the generality of the phenomenon we have discovered, as it can be adapted to solids, liquids, and gases.

"Place the substance to be experimented with in a glass test-tube, connect a rubber tube with the mouth of the test-tube, placing the other end of the pipe to the ear. Then focus the intermittent beam upon the substance in the tube. I have tried a large number of substances in this way with great success, although it is extremely difficult to get a glimpse of the sun here, and when it does shine the intensity of the light is not to be compared with that to be obtained in Washington. I got splendid effects from crystals of bichromate of potash, crystals of sulphate of copper, and from tobacco smoke. A whole cigar placed in the test-

Fig. 1.

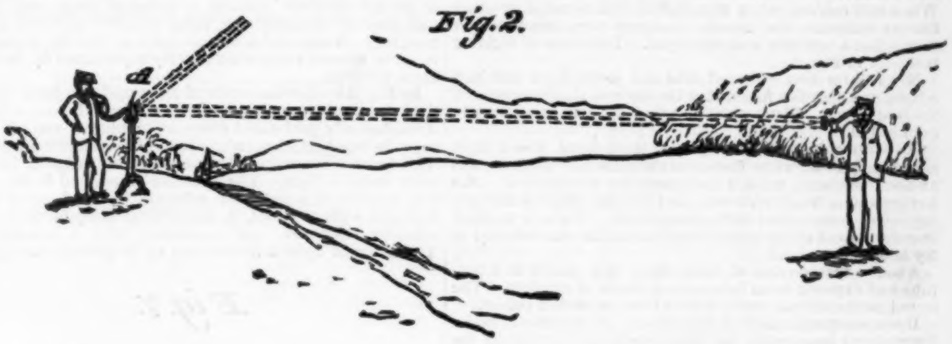


Fig. 2.

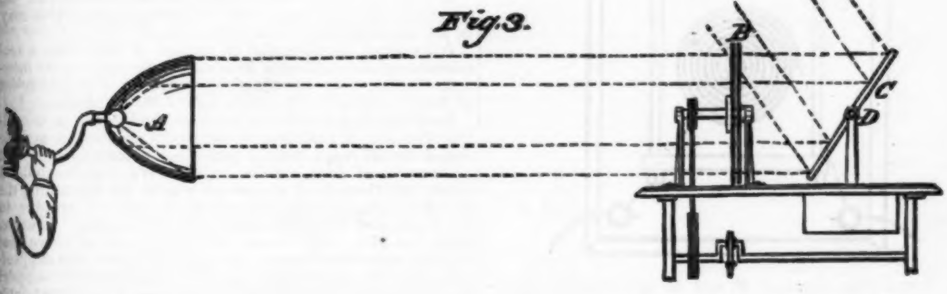


Fig. 3.

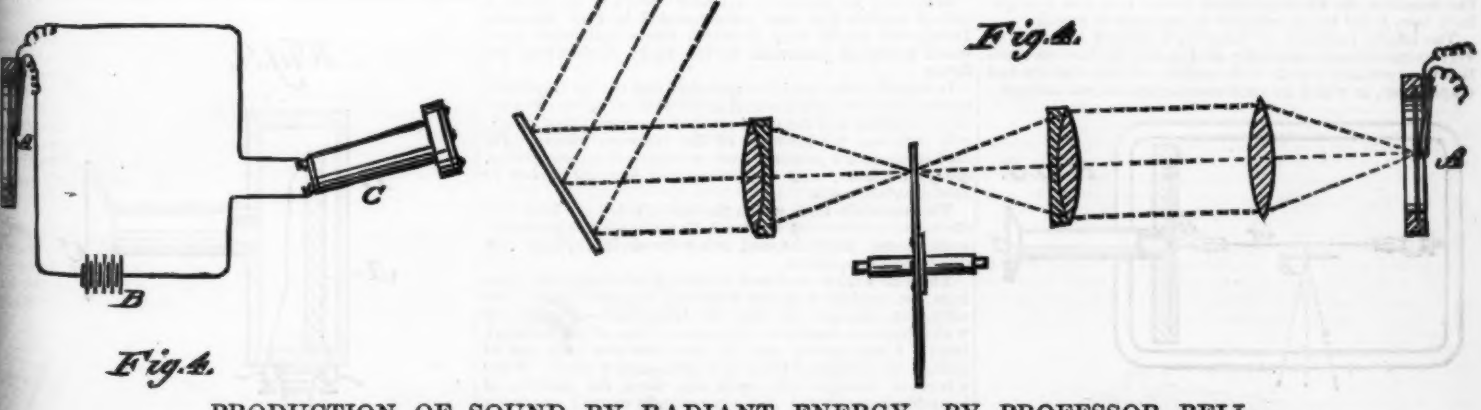


Fig. 4.

Fig. 4.

PRODUCTION OF SOUND BY RADIANT ENERGY.—BY PROFESSOR BELL.

* A paper read before the National Academy of Arts and Sciences, April 21, 1881.

† Proceedings of American Association for the Advancement of Science August 27, 1880; see also, *American Journal of Science*, vol. xx., p. 305; *Journal of the American Electrical Society*, vol. iii., p. 3; *Journal of the Society of Telegraph Engineers and Electricians*, vol. ix., p. 404; *Annales de Chimie et de Physique*, vol. xxi.

* *Comptes Rendus*, vol. xci., p. 595.

* "Notes on Radiophony," *Comptes Rendus*, Dec 6 and 13, 1880; Feb. 21 and 28, 1881. See, also, *Journal de Physique*, vol. x., p. 53.

† "Action of an Intermittent Beam of Radiant Heat upon Gaseous Matter." *Proc. Royal Society*, Jan. 13, 1881, vol. xxxi., p. 307.

‡ "On the Tones which Arise from the Intermittent Illumination of a Gas." See *Annalen der Phys. und Chemie*, Jan., 1881, No. 1, p. 155.

§ "On the Conversion of Radiant Energy into Sonorous Vibrations." *Proc. Royal Society*, March 10, 1881, vol. xxxi., p. 506.

tube produced a very loud sound. I could not hear anything from plain water, but when the water was discolored with ink a feeble sound was heard. I would suggest that you might repeat these experiments and extend the results," etc., etc.

Upon my return to Washington in the early part of January,* Mr. Tainter communicated to me the results of the experiments he had made in my laboratory during my absence in Europe.

He had commenced by examining the sonorous properties of a vast number of substances inclosed in test-tubes in a simple empirical search for loud effects. He was thus led gradually to the discovery that cotton-wool, worsted, silk, and fibrous materials generally, produced much louder sounds than hard rigid bodies like crystals, or diaphragms such as we had hitherto used.

In order to study the effects under better circumstances he inclosed his materials in a conical cavity in a piece of brass closed by a flat plate of glass. A brass tube leading into the cavity served for connection with the hearing-tube. When this conical cavity was stuffed with worsted or other fibrous materials the sounds produced were much louder than when a test-tube was employed. This form of receiver is shown in Fig. 1.

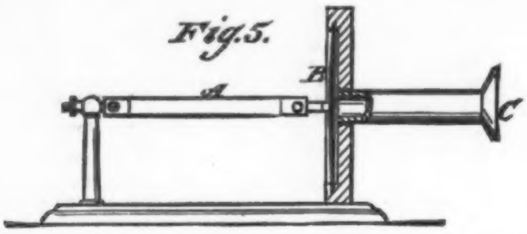


Fig. 5.

Mr. Tainter next collected silks and worsteds of different colors, and speedily found that the darkest shades produced the best effects. Black worsted especially gave an extremely loud sound.

As white cotton-wool had proved itself equal, if not superior, to any other white fibrous material before tried, he was anxious to obtain colored specimens for comparison. Not having any at hand, however, he tried the effect of darkening some cotton-wool with lamp-black. Such a marked re-enforcement of the sound resulted that he was induced to try lamp-black alone.

About a teaspoonful of lamp-black was placed in a test-tube and exposed to an intermittent beam of sunlight. The sound produced was much louder than any heard before.

Upon smoking a piece of plate-glass, and holding it in the intermittent beam with the lamp-black surface toward the sun, the sound produced was loud enough to be heard, with attention, in any part of the room. With the lamp-black surface turned from the sun the sound was much feebler.

Mr. Tainter repeated these experiments for me immediately upon my return to Washington, so that I might verify his results.

Upon smoking the interior of the conical cavity shown in Fig. 1, and then exposing it to the intermittent beam, with the glass lid in position as shown, the effect was perfectly startling. The sound was so loud as to be actually painful to an ear placed closely against the end of the hearing-tube.

The sounds, however, were sensibly louder when we placed some smoked wire gauze in the receiver, as illustrated in the drawing, Fig. 1.

When the beam was thrown into a resonator, the interior of which had been smoked over a lamp, most curious alternations of sound and silence were observed. The interrupting disk was set rotating at a high rate of speed, and was then allowed to come gradually to rest. An extremely feeble musical tone was at first heard, which gradually fell in pitch as the rate of interruption grew less. The loudness of the sound produced varied in the most interesting manner. Minor re-enforcements were constantly occurring, which became more and more marked as the true pitch of the resonator was neared. When at last the frequency of interruption corresponded to the frequency of the fundamental of the resonator, the sound produced was so loud that it might have been heard by an audience of hundreds of people.

The effects produced by lamp-black seemed to me to be very extraordinary, especially as I had a distinct recollection of experiments made in the summer of 1880 with smoked diaphragms, in which no such re-enforcement was noticed.

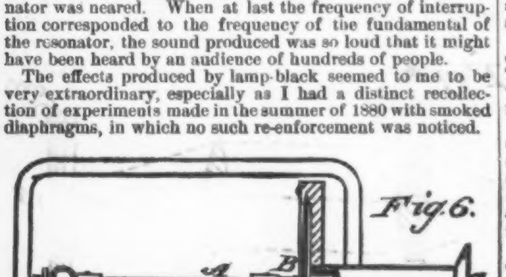


Fig. 6.

Upon examining the records of our past photophonic experiments we found in vol. vii., p. 57, the following note:

"Experiment V.—Mica diaphragm covered with lamp-black on side exposed to light.

"Result: distinct sound about same as without lamp-black.—*A. G. B., July* 18, 1880.

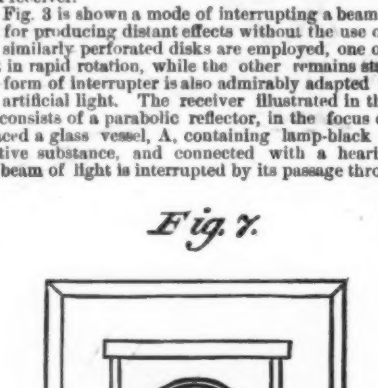
"Verified the above, but think it somewhat louder than when used without lamp-black."—*S. T., July* 18, 1880.

Upon repeating this old experiment we arrived at the same result as that noted. Little if any augmentation of sound resulted from smoking the mica. In this experiment the effect was observed by placing the mica diaphragm against the ear and also by listening through a hearing-tube, one end of which was closed by the diaphragm. The sound was found to be more audible through the free air when the ear was placed as near to the lamp-black surface as it could be brought without shading it.

At the time of my communication to the American Association I had been unable to satisfy myself that the substances which had become sonorous under the direct influence of intermittent sunlight were capable of reproducing the sounds of articulate speech under the action of an undulatory beam from our photophonic transmitter. The difficulty in ascertaining this will be understood by considering that the sounds emitted by thin diaphragms and tubes were so feeble that it was impracticable to produce audible effects from substances in these conditions at any considerable distance away from the transmitter; but it was equally impossible to judge of the effects produced by our regular articulate transmitter at a short distance away, because the speaker's voice was directly audible through the air. The extremely loud sounds produced from lamp-black have enabled us to demonstrate the feasibility of using this substance in an articulating photophone in place of the electrical receiver formerly employed.

The drawing, Fig 2, illustrates the mode in which the experiment was conducted. The diaphragm of the transmitter, A, was only 5 centimeters in diameter, the diameter of the receiver, B, was also 5 centimeters, and the distance between the two was 40 meters, or 800 times the diameter of the transmitting diaphragm. We were unable to experiment at greater distances without a heliostat on account of the difficulty of keeping the light steadily directed on the receiver. Words and sentences spoken into the transmitter in a low tone of voice were audibly reproduced by the lamp-black receiver.

In Fig. 3 is shown a mode of interrupting a beam of sunlight for producing distant effects without the use of lenses. Two similarly perforated disks are employed, one of which is set in rapid rotation, while the other remains stationary. This form of interrupter is also admirably adapted for work with artificial light. The receiver illustrated in the drawing, consists of a parabolic reflector, in the focus of which is placed a glass vessel, A, containing lamp-black or other sensitive substance, and connected with a hearing-tube. The beam of light is interrupted by its passage through the

Fig. 7.


two slotted disks shown at B, and in operating the instrument musical signals like the dots and dashes of the Morse alphabet are produced from the sensitive receiver, A, by slight motions of the mirror, C, about its axis, D.

In place of the parabolic reflector shown in the figure a conical reflector like that recommended by Prof. Sylvanus Thompson* can be used, in which case a cylindrical glass vessel would be preferable to the flask, A, shown in the figure

In regard to the sensitive materials that can be employed, our experiments indicate that in the case of solids the physical condition and the color are two conditions that markedly influence the intensity of the sonorous effects. *The loudest sounds are produced from substances in a loose, porous, spongy condition, and from those that have the darkest or most absorbent colors.*

The materials from which the best effects have been produced are cotton-wool worsted, fibrous materials generally, cork, sponge, platinum, and other metals in a spongy condition, and lamp-black.

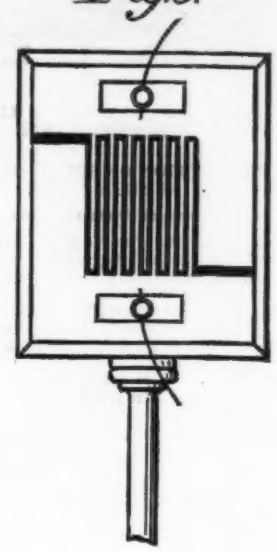
The loud sounds produced from such substances may, perhaps, be explained in the following manner: Let us consider, for example, the case of lamp-black—a substance which becomes heated by exposure to rays of all refrangibility. I look upon a mass of this substance as a sort of sponge, with its pores filled with air instead of water. When a beam of sunlight falls upon this mass, the particles of lamp-black are heated, and consequently expand, causing a contraction of the air-spaces or pores among them.

Under these circumstances a pulse of air should be expelled, just as we would squeeze out water from a sponge.

The force with which the air is expelled must be greatly increased by the expansion of the air itself, due to contact with the heated particles of lamp-black. When the light is cut off the converse process takes place. The lamp-black particles cool and contract, thus enlarging the air spaces among them, and the inclosed air also becomes cool. Under these circumstances a partial vacuum should be formed among the particles, and the outside air would then be absorbed, as water is by a sponge when the pressure of the hand is removed.

I imagine that in some such manner as this a wave of condensation is started in the atmosphere each time a beam of sunlight falls upon lamp-black and a wave of rarefaction is originated when the light is cut off. *We can thus understand how it is that a substance like lamp-black produces intense sonorous vibrations in the surrounding air, while at the same time it communicates a very feeble vibration to the diaphragm or solid bed upon which it rests.*

This curious fact was independently observed in England by Mr. Preece, and it led him to question whether, in our experiments with thin diaphragms, the sound heard was due to the vibration of the disk or (as Prof. Hughes had suggested) to the expansion and contraction of the air in contact with the disk confined in the cavity behind the diaphragm. In his paper, read before the Royal Society on the 10th of March, Mr. Preece describes experiments from which he claims to have proved that the effects are wholly due to

Fig. 8.


the vibrations of the confined air, and that the *disks do not vibrate at all.*

I shall briefly state my reasons for disagreeing with him in this conclusion:

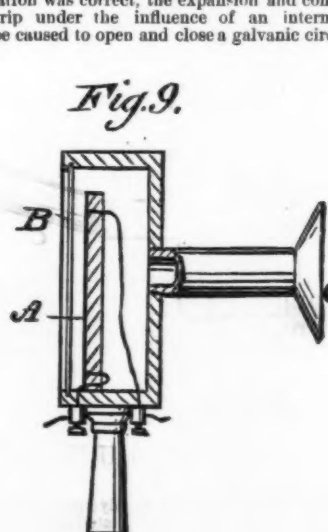
1. When an intermittent beam of sunlight is focused upon a sheet of hard rubber or other material, a musical tone can be heard, not only by placing the ear immediately behind the part receiving the beam, but by placing it against any portion of the sheet, even though this may be a foot or more from the place acted upon by the light.

2. When the beam is thrown upon the diaphragm of a "Blake Transmitter," a loud musical tone is produced by a telephone connected in the same galvanic circuit with the carbon button, A, Fig. 4. Good effects are also produced when the carbon button, A, forms, with the battery, B, a portion of the primary circuit of an induction coil, the telephone, C, being placed in the secondary circuit.

In these cases the wooden box and mouth-piece of the transmitter should be removed so that no air cavities may be left on either side of the diaphragm.

It is evident, therefore, that in the case of thin disks a real vibration of the diaphragm is caused by the action of the intermittent beam, independently of any expansion and contraction of the air confined in the cavity behind the diaphragm.

Lord Rayleigh has shown mathematically that a to-and-fro vibration, of sufficient amplitude to produce an audible sound, would result from a periodical communication and abstraction of heat, and he says: "We may conclude, I think, that there is at present no reason for discarding the obvious explanation that the sounds in question are due to the bending of the plates under unequal heating." (*Nature*, xxiii., p. 274.) Mr. Preece, however, seeks to prove that the sonorous effects cannot be explained upon this supposition; but his experimental proof is inadequate to support his conclusion. Mr. Preece expected that if Lord Rayleigh's explanation was correct, the expansion and contraction of a thin strip under the influence of an intermittent beam could be caused to open and close a galvanic circuit so as to

Fig. 9.


produce a musical tone from a telephone in the circuit. But this was an inadequate way to test the point at issue, for Lord Rayleigh has shown (Proc of Roy. Soc., 1877) that an audible sound can be produced by a vibration whose amplitude is *less than a ten-millionth of a centimeter*, and certainly such a vibration as that would not have sufficed to operate a "make-and-break contact" like that used by Mr. Preece. The negative results obtained by him cannot, therefore, be considered conclusive.

The following experiments (devised by Mr. Tainter) have given results decidedly more favorable to the theory of Lord Rayleigh than to that of Mr Preece:

1. A strip, A, similar to that used in Mr. Preece's experi-

* On the 7th of January.

* *Phil. Mag.*, April, 1881, vol. xi., p. 286.

ment, was attached firmly to the center of an iron diaphragm, B, as shown in Fig. 5, and was then pulled taut at right angles to the plane of the diaphragm. When the intermittent beam was focused upon the strip, A, a clear musical tone could be heard by applying the ear to the hearing-tube, C.

This seemed to indicate a rapid expansion and contraction of the substance under trial.

But a vibration of the diaphragm, B, would also have resulted if the thin strip, A, had acquired a to-and-fro motion, due either to the direct impact of the beam or to the sudden expansion of the air in contact with the strip.

2. To test whether this had been the case an additional strip, D, was attached by its central point only to the strip under trial, and was then submitted to the action of the beam, as shown in Fig. 6.

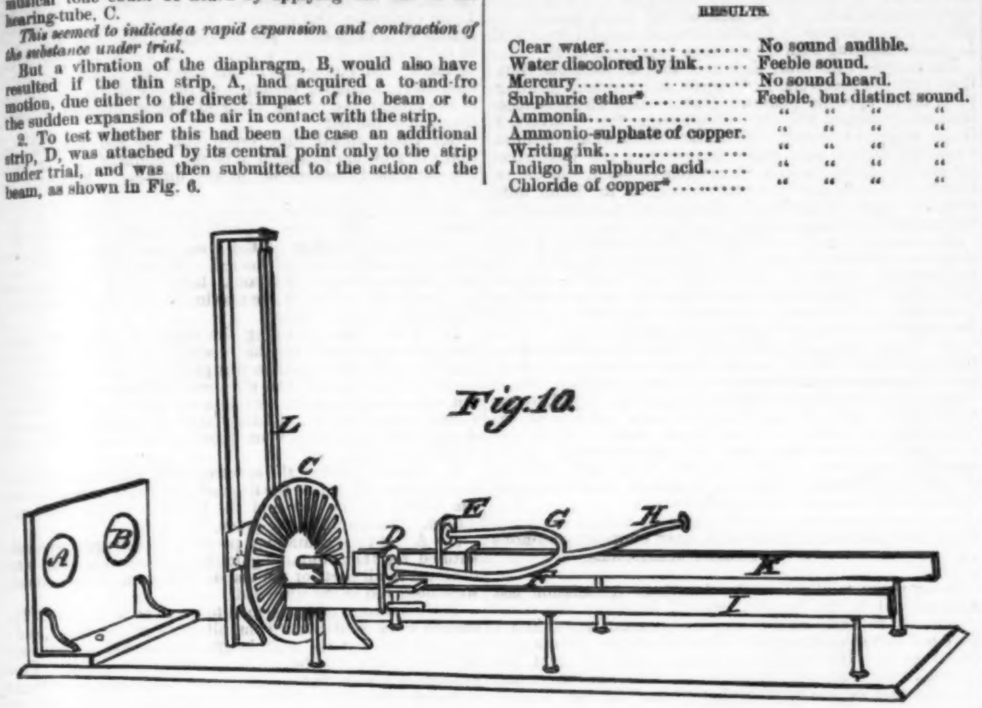


Fig. 10.

It was presumed that if the vibration of the diaphragm, B, had been due to a *pushing force* acting on the strip, A, that the addition of the strip, D, would not interfere with the effect. But if, on the other hand, it had been due to the longitudinal expansion and contraction of the strip, A, the sound would cease, or at least be reduced. The beam of light falling upon strip, D, was now interrupted as before by the rapid rotation of a perforated disk, which was allowed to come gradually to rest.

No sound was heard excepting at a certain speed of rotation, when a feeble musical tone became audible.

This result is confirmatory of the first.

The audibility of the effect at a particular rate of interruption suggests the explanation that the strip, D, had a normal rate of vibration of its own.

When the frequency of the interruption of the light corresponded to this, the strip was probably thrown into vibration after the manner of a tuning-fork, in which case a to-and-fro vibration would be propagated down its stem or central support to the strip, A.

This indirectly proves the value of the experiment.

The list of solid substances that have been submitted to experiment in my laboratory is too long to be quoted here, and I shall merely say that we have not yet found one solid body that has failed to become sonorous under proper conditions of experiment.*

EXPERIMENTS WITH LIQUIDS.

The sounds produced by liquids are much more difficult to observe than those produced by solids. The high absorptive power possessed by most liquids would lead one to expect intense vibrations from the action of intermittent light, but the number of sonorous liquids that have so far been found is extremely limited, and the sounds produced are so feeble as to be heard only by the greatest attention and under the best circumstances of experiment. In the experiments made in my laboratory a very long test-tube was filled with the liquid under examination, and a flexible rubber tube was slipped over the mouth far enough down to prevent the possibility of any light reaching the vapor above the surface. Precautions were also taken to prevent reflection from the bottom of the test-tube. An intermittent beam of sunlight was then focused upon the liquid in the middle portion of the test-tube by means of a lens of large diameter.

RESULTS.

Clear water	No sound audible.
Water discolored by ink	Feeble sound.
Mercury	No sound heard.
Sulphuric ether*	Feeble, but distinct sound.
Ammonia	" " " "
Ammonio-sulphate of copper	" " " "
Writing ink	" " " "
Indigo in sulphuric acid	" " " "
Chloride of copper*	" " " "

The liquids distinguished by an asterisk gave the best sounds.

Acoustic vibrations are also much enfeebled in passing from liquids to gases, and it is probable that a form of experiment may be devised which will yield better results by communicating the vibrations of the liquid to the ear through the medium of a solid rod.

EXPERIMENTS WITH GASEOUS MATTER.

On the 29th of November, 1880, I had the pleasure of showing to Prof. Tyndall in the laboratory of the Royal Institution the experiments described in the letter to Mr Tainter from which I have quoted above, and Prof. Tyndall at once expressed the opinion that the sounds were due to rapid changes of temperature in the body submitted to the action of the beam. Finding that no experiments had been made at that time to test the sonorous properties of different gases, he suggested filling one test-tube with the vapor of sulphuric ether (a good absorbent of heat), and another with the vapor of bisulphide of carbon (a poor absorbent), and he predicted that if any sound was heard it would be louder in the former case than in the latter.

The experiment was immediately made, and the result verified the prediction.

Since the publication of the memoirs of Röntgen* and Tyndall† we have repeated these experiments, and have extended the inquiry to a number of other gaseous bodies, obtaining in every case similar results to those noted in the memoirs referred to.

The vapors of the following substances were found to be highly sonorous in the intermittent beam: Water vapor, coal gas, sulphuric ether, alcohol, ammonia, amylene, ethyl bromide, diethylamene, mercury, iodine, and peroxide of nitrogen. The loudest sounds were obtained from iodine and peroxide of nitrogen.

I have now shown that sounds are produced by the direct action of intermittent sunlight from substances in every physical condition (solid, liquid, and gaseous), and the probability is therefore very greatly increased that sonorousness under such circumstances will be found to be a universal property of matter.

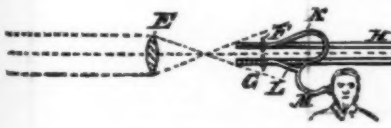
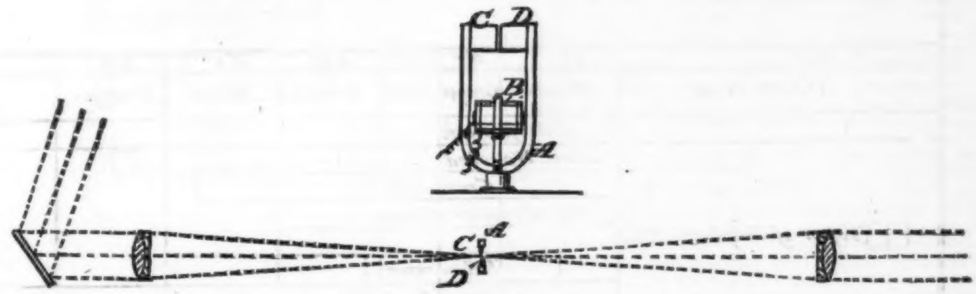


Fig. 11.



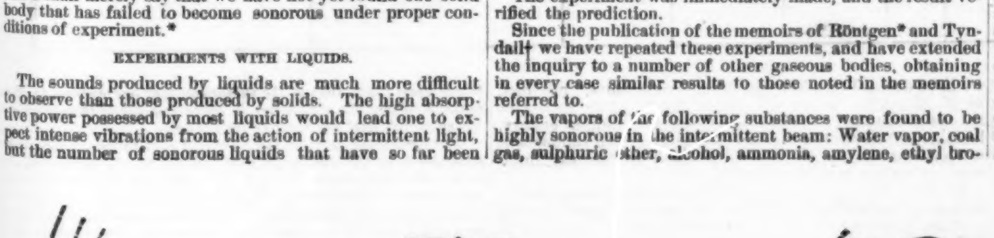


Fig. 12.

UPON SUBSTITUTES FOR SELENIUM IN ELECTRICAL RECEIVERS.

At the time of my communication to the American Association the loudest effects obtained were produced by the use of selenium, arranged in a cell of suitable construction, and placed in a galvanic circuit with a telephone. Upon allowing an intermittent beam of sunlight to fall upon the selenium a musical tone of great intensity was produced from the telephone connected with it.

But the selenium was very inconstant in its action. It was rarely, if ever, found to be the case, that two pieces of selenium (even of the same stick) yielded the same results under identical circumstances of annealing, etc. While in Europe last autumn, Dr. Chichester Bell, of University College, London, suggested to me that this inconstancy of result might be due to chemical impurities in the selenium used. Dr. Bell has since visited my laboratory in Washington, and has made a chemical examination of the various samples of selenium I had collected from different parts of the world. As I understand it to be his intention to publish the results of this analysis very soon, I shall make no further mention of his investigation than to state that he has found sulphur, iron, lead, and arsenic in the so-called "selenium," with traces of organic matter; that a quantitative examination has revealed the fact that sulphur constitutes nearly one per cent. of the whole mass; and that when these impurities are eliminated the selenium appears to be more constant in its action and more sensitive to light.

Prof. W. G. Adams* has shown that tellurium, like selenium, has its electrical resistance affected by light, and we have attempted to utilize this substance in place of selenium. The arrangement of cell (shown in Fig. 7) was constructed for this purpose in the early part of 1880; but we failed at that time to obtain any indications of sensitiveness with a reflecting galvanometer. We have since found, however, that when this tellurium spiral is connected in circuit with a galvanic battery and telephone, and exposed to the action of an intermittent beam of sunlight, a distinct musical tone is produced by the telephone. The audible effect is much increased by placing the tellurium cell with the battery in the primary circuit of an induction coil, and placing the telephone in the secondary circuit.

The enormously high resistance of selenium and the extremely low resistance of tellurium suggested the thought that an alloy of these two substances might possess intermediate electrical properties. We have accordingly mixed together selenium and tellurium in different proportions, and while we do not feel warranted at the present time in making definite statements concerning the results, I may say that such alloys have proved to be sensitive to the action of light.

It occurred to Mr. Tainter before my return to Washington, last January, that the very great molecular disturbance produced in lamp-black by the action of intermittent sunlight should produce a corresponding disturbance in an electric current passed through it, in which case lamp-black could be employed in place of selenium in an electrical receiver. This has turned out to be the case, and the importance of the discovery is very great, especially when we consider the expense of such rare substances as selenium and tellurium.

The form of lamp-black cell we have found most effective is shown in Fig. 8. Silver is deposited upon a plate of glass, and a zigzag line is then scratched through the film, as shown, dividing the silver surface into two portions insulated from one another, having the form of two combs with interlocking teeth.

Each comb is attached to a screw-cup, so that the cell can be placed in an electrical circuit when required. The surface is then smoked until a good film of lamp-black is obtained, filling the interstices between the teeth of the silver combs. When the lamp-black cell is connected with a telephone and galvanic battery, and exposed to the influence of an intermittent beam of sunlight, a loud musical tone is produced by the telephone. This result seems to be due rather to the physical condition than to the nature of the conducting material employed, as metals in a spongy condition produce similar effects. For instance, when an electrical current is passed through spongy platinum while it is exposed to intermittent sunlight, a distinct musical tone is produced by a telephone in the same circuit. In all such cases the effect is increased by the use of an induction coil; and the sensitive cells can be employed for the reproduction of articulate speech as well as for the production of musical sounds.

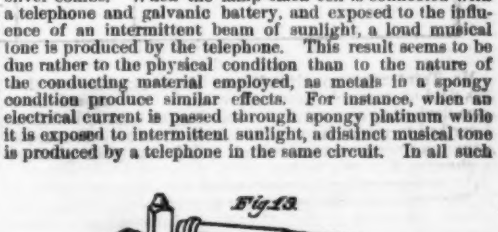


Fig. 13.

We have also found that loud sounds are produced from lamp-black by passing through it an intermittent electrical current; and that it can be used as a telephonic receiver for the reproduction of articulate speech by electrical means.

A convenient mode of arranging a lamp-black cell for experimental purposes is shown in Fig. 9. When an inter-

* Carbon and thin microscope glass are mentioned in my Boston paper as non-responsive, and powdered chlorate of potash in the communication to the French Academy (*Comptes Rendus*, vol. xci., p. 595). All these substances have since yielded sounds under more careful conditions of experiment.

* *Ann. der Phys. und Chem.*, 1881, No. 1, p. 155.

† *Proc. Roy. Soc.*, vol. xxxi., p. 307.

* *Proc. Roy. Soc.*, vol. xxiv., p. 163.

mittent current is passed through the lamp-black, A, or when an intermittent beam of sunlight falls upon it through the glass-plate, B, a loud musical tone can be heard by applying the ear to the hearing-tube, C. When the light and the electrical current act simultaneously, two musical tones are perceived, which produce beats when nearly of the same pitch. By proper arrangements a complete interference of sound can undoubtedly be produced.

UPON THE MEASUREMENT OF THE SONOROUS EFFECTS PRODUCED BY DIFFERENT SUBSTANCES.

We have observed that different substances produce sounds of very different intensities under similar circumstances of experiment, and it has appeared to us that very valuable information might be obtained if we could measure the audible effects produced. For this purpose we have constructed several different forms of apparatus for studying the effects, but as our researches are not yet complete, I shall confine myself to a simple description of some of the forms of apparatus we have devised.

When a beam of light is brought to a focus by means of a lens, the beam diverging from the focal point becomes weaker as the distance increases in a calculable degree. Hence, if we can determine the distances from the focal point at which two different substances emit sounds of equal intensity, we can calculate their relative sonorous powers.

Preliminary experiments were made by Mr. Tainter during my absence in Europe, to ascertain the distance from the focal point of a lens at which the sound produced by a substance became inaudible. A few of the results obtained will show the enormous differences existing between different substances in this respect.

DISTANCE FROM FOCAL POINT OF LENS AT WHICH SOUNDS BECOME INAUDIBLE WITH DIFFERENT SUBSTANCES.

Zinc diaphragm (polished)		1·51 m.
Hard rubber diaphragm		1·90 "
Tin-foil "		2·00 "
Telephone "	(japanned iron)	2·15 "
Zinc "	(unpolished)	2·15 "
White silk,	(In receiver shown in Fig. 1.)	3·10 "
White worsted,	" " " "	4·01 "
Yellow worsted,	" " " "	4·06 "
Yellow silk,	" " " "	4·13 "
White cotton-wool,	" " " "	4·38 "
Green silk,	" " " "	4·52 "
Blue worsted,	" " " "	4·69 "
Purple silk,	" " " "	4·82 "
Brown silk,	" " " "	5·03 "
Black silk,	" " " "	5·21 "
Red silk,	" " " "	5·24 "
Black worsted,	" " " "	6·50 "
Lamp-black. In receiver the limit of audibility could not be determined on account of want of space. Sound perfectly audible at a distance of		10.00 "

tion, it is placed at a sufficient distance away to be inaudible through the air, and a system of lenses is employed for the purpose of bringing the undulating beam of light to the receiving lens, E, with as little loss as possible. The two receivers, F G, are attached to slides, H I, which move upon opposite sides of the axis of the beam, and the receivers are connected by flexible tubes of unequal length, K L, communicating with the common hearing-tube, M.

The length of the tube, K, is such that the sonorous vibrations from the receivers, F G, reach the common hearing-tube, M, in opposite phases. Under these circumstances silence is produced when the vibrations in the receivers, F G, are of equal intensity. When the intensities are unequal, a residual effect is perceived. In operating the instrument the position of the receiver, G, remains constant, and the receiver, F, is moved to or from the focus of the beam until complete silence is produced. The relative positions of the two receivers are then noted.

(3.) Another mode is as follows: The loudness of a musical tone produced by the action of light is compared with the loudness of a tone of similar pitch produced by electrical means. A rheostat introduced into the circuit enables us to measure the amount of resistance required to render the electrical sound equal in intensity to the other.

(4.) If the tuning-fork, A, in Fig. 11, is thrown into vibration by an undulatory instead of an intermittent current passed through the electro-magnet, B, it is probable that a musical tone, electrically produced in the receiver, F, by the action of the same current, would be found capable of extinguishing the effect produced in the receiver, G, by the action of the undulatory beam of light, in which case it should be possible to establish an acoustic balance between the effects produced by light and electricity by introducing sufficient resistance into the electric circuit.

UPON THE NATURE OF THE RAYS THAT PRODUCE SONOROUS EFFECTS IN DIFFERENT SUBSTANCES.

In my paper read before the American Association last August, and in the present paper, I have used the word "light" in its usual rather than its scientific sense, and I have not hitherto attempted to discriminate the effects produced by the different constituents of ordinary light, the thermal, luminous, and actinic rays. I find, however, that the adoption of the word "photophone" by Mr. Tainter and myself has led to the assumption that we believed the audible effects discovered by us to be due entirely to the action of luminous rays. The meaning we have uniformly attached to the words "photophone" and "light" will be obvious from the following passage, quoted from my Boston paper:

"Although effects are produced as above shown by forms of radiant energy, which are invisible, we have named the apparatus for the production and reproduction of sound in this way the 'photophone,' *because an ordinary beam of light contains the rays which are operative.*"

To avoid in future any misunderstandings upon this point

decrease, and then stopped so suddenly that a very slight motion of the receiver, G, made all the difference between almost maximum sound and complete silence.

(2.) The lamp-blacked wire gauze was then removed and the interior of the receiver, G, was filled with red worsted. Upon exploring the spectrum as before, entirely different results were obtained. The maximum effect was produced in the green at that part where the red worsted appeared to be black. On either side of this point the sound gradually died away, becoming inaudible on the one side in the middle of the indigo, and on the other at a short distance outside the edge of the red.

(3.) Upon substituting green silk for red worsted the limits of audition appeared to be the middle of the blue and a point a short distance out in the ultra-red. Maximum in the red.

(4.) Some hard rubber shavings were now placed in the receiver, G. The limits of audibility appeared to be on the one hand the junction of the green and blue, and on the other the outside edge of the red. Maximum in the yellow. Mr. Tainter thought he could hear a little way into the ultra-red, and to his ear the maximum was about the junction of the red and orange.

(5.) A test-tube containing the vapor of sulphuric ether was then substituted for the receiver, G. Commencing at the violet end, the test-tube was gradually moved down the spectrum and out into the ultra-red without audible effect, but when a certain point far out in the ultra-red was reached, a distinct musical tone suddenly made its appearance, which disappeared as suddenly on moving the test-tube a very little further on.

(6.) Upon exploring the spectrum with a test-tube containing the vapor of iodine the limits of audibility appeared to be the middle of the red and the junction of the blue and indigo. Maximum in the green.

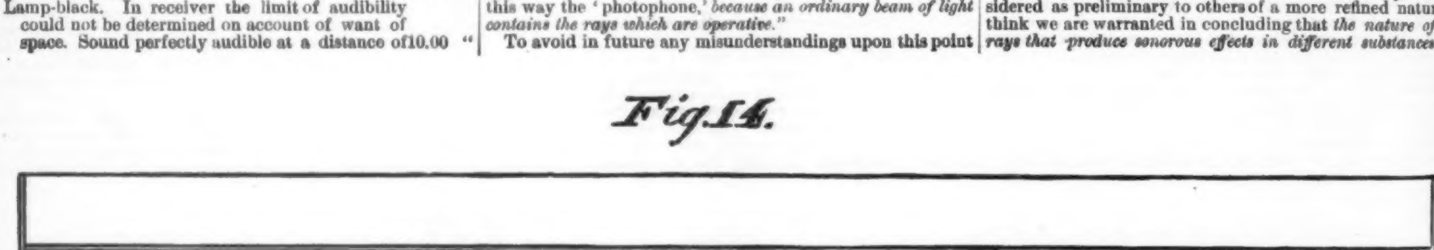
(7.) A test-tube containing peroxide of nitrogen was substituted for that containing iodine. Distinct sounds were obtained in all parts of the visible spectrum, but no sounds were observed in the ultra-red..

The maximum effect seemed to me to be in the blue. The sounds were well-marked in all parts of the violet, and I even fancied that the audible effect extended a little way into the ultra-violet, but of this I cannot be certain. Upon examining the absorption spectrum of peroxide of nitrogen it was at once observed that the maximum sound was produced in that part of the spectrum where the greatest number of absorption lines made their appearance.

(8.) The spectrum was now explored by a selenium cell and the audible effects were observed by means of a telephone in the same galvanic circuit with the cell. The maximum effect was produced in the red. The audible effect extended a little way into the ultra-red on the one hand, and up as high as the middle of the violet on the other.

Although the experiments so far made can only be considered as preliminary to others of a more refined nature, I think we are warranted in concluding that *the nature of the rays that produce sonorous effects in different substances depends upon the nature of the substances that are exposed to the beam, and that the sounds are in every case due to those rays of the spectrum that are absorbed by the body.*

Fig. 14.



Mr. Tainter was convinced from these experiments that this field of research promised valuable results, and he at once devised an apparatus for studying the effects, which he described to me upon my return from Europe. The apparatus has since been constructed, and I take great pleasure in showing it to you to-day.

(1.) A beam of light is received by two similar lenses, A B, Fig. 10, which bring the light to a focus on either side of the interrupting disk, C. The two substances, whose sonorous powers are to be compared, are placed in the receiving vessels, D E (so arranged as to expose equal surfaces to the action of the beam), which communicate by flexible tubes, F G, of equal length, with the common hearing-tube, H. The receivers, D E, are placed upon slides, which can be removed along the graduated supports, I K. The beams of light passing through the interrupting disk, C, are alternately cut off by the swinging of a pendulum, L. Thus a musical tone is produced alternately from the substance in D, and from that in E. One of the receivers is kept at a constant point upon its scale, and the other receiver is moved toward or from the focus of its beam until the ear decides that the sounds produced from D and E are of equal intensity. The relative positions of the receivers are then noted.

(2.) Another method of investigation is based upon the production of an interference of sound, and the apparatus employed is shown in Fig. 11. The interrupter consists of a tuning-fork, A, which is kept in continuous vibration by means of an electro-magnet, B.

A powerful beam of light is brought to a focus between the prongs of the tuning-fork, A, and the passage of the beam is more or less obstructed by the vibration of the opaque screens, C D, carried by the prongs of the fork.

As the tuning-fork, A, produces a sound by its own vibra-

we have decided to adopt the term "*radiophone*," proposed by M. Mercadier, as a general term signifying an apparatus for the production of sound by any form of radiant energy, limiting the words *thermophone*, *photophone*, and *actinophone* to apparatus for the production of sound by thermal, luminous, or actinic rays respectively.

M. Mercadier, in the course of his researches in radiophony, passed an intermittent beam from an electric lamp through a prism, and then examined the audible effects produced in different parts of the spectrum (*Comptes Rendus*, Dec. 6, 1880).

We have repeated this experiment, using the sun as our source of radiation, and have obtained results somewhat different from those noted by M. Mercadier.

A beam of sunlight was reflected from a heliostat, A, Fig. 12, through an achromatic lens, B, so as to form an image of the sun upon the slit, C.

The beam then passed through another achromatic lens, D, and through a bisulphide of carbon prism, E, forming a spectrum of great intensity, which, when focused upon a screen, was found to be sufficiently pure to show the principal absorption lines of the solar spectrum.

The disk-interrupter, F, was then turned with sufficient rapidity to produce from five to six hundred interruptions of the light per second, and the spectrum was explored with the receiver, G, which was so arranged that the lamp-black surface exposed was limited by a slit, as shown.

Under these circumstances sounds were obtained in every part of the visible spectrum, excepting the extreme half of the violet, as well as in the ultra-red. A continuous increase in the loudness of the sound was observed upon moving the receiver, G, gradually from the violet into the ultra-red. The point of maximum sound lay very far out in the ultra-red. Beyond this point the sound began to

Our experiments upon the range of audibility of different substances in the spectrum have led us to the construction of a new instrument for use in spectrum analysis, which was described and exhibited to the Philosophical Society of Washington last Saturday.* The eye-piece of a spectroscope is removed, and sensitive substances are placed in the focal point of the instrument behind an opaque diaphragm containing a slit. These substances are put in communication with the ear by means of a hearing-tube, and thus the instrument is converted into a veritable "spectrophone," like that shown in Fig. 13.

Suppose we smoke the interior of our spectrophonic receiver, and fill the cavity with peroxide of nitrogen gas. We have then a combination that gives us good sounds in all parts of the spectrum (visible and invisible), except the ultra-violet. Now pass a rapidly interrupted beam of light through some substance whose absorption spectrum is to be investigated, and bands of sound and silence are observed upon exploring the spectrum, the silent positions corresponding to the absorption bands. Of course, the ear cannot for one moment compete with the eye in the examination of the visible part of the spectrum; but in the invisible part beyond the red, where the eye is useless, the ear is invaluable. In working in this region of the spectrum, lamp-black alone may be used in the spectrophonic receiver. Indeed, the sounds produced by this substance in the ultra-red are so well marked as to constitute our instrument a most reliable and convenient substitute for the thermopile. A few experiments that have been made may be interesting.

* Proc. of Phil. Soc. of Washington, April 16, 1881.

(1.) The interrupted beam was filtered through a saturated solution of alum.

Result: The range of audibility in the ultra-red was slightly reduced by the absorption of a narrow band of the rays of lowest refrangibility. The sounds in the visible part of the spectrum seemed to be unaffected.

(2.) A thin sheet of hard rubber was interposed in the path of the beam.

Result: Well marked sounds in every part of the ultra-red. No sounds in the visible part of the spectrum, excepting the extreme half of the red.

These experiments reveal the cause of the curious fact alluded to in my paper read before the American Association last August—that sounds were heard from selenium when the beam was filtered through both hard rubber and alum at the same time. (See table of results in Fig. 14.)

(3.) A solution of ammonia-sulphate of copper was tried.

Result: When placed in the path of the beam the spectrum disappeared, with the exception of the blue and violet end. To the eye the spectrum was thus reduced to a single broad band of blue-violet light. To the ear, however, the spectrum revealed itself as two bands of sound with a broad space of silence between. The invisible rays transmitted constituted a narrow band just outside the red.

I think I have said enough to convince you of the value of this new method of examination, but I do not wish you to understand that we look upon our results as by any means complete. It is often more interesting to observe the first totterings of a child than to watch the firm tread of a full-grown man, and I feel that *our* first footsteps in this new field of science may have more of interest to you than the fuller results of mature research. This must be my excuse for having dwelt so long upon the details of incomplete experiments.

I recognize the fact that the spectrophone must ever remain a mere adjunct to the spectroscope, but I anticipate that it has a wide and independent field of usefulness in the investigation of absorption spectra in the ultra-red.

THE GOWER-BELL TELEPHONE.

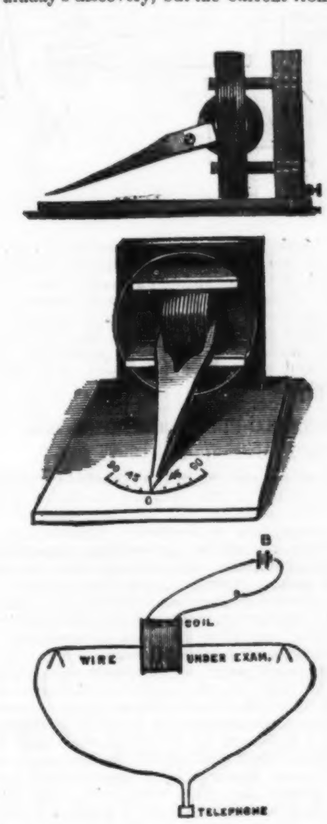
The modification of the Bell telephone adopted by the British Postal Service, and known as the Gower-Bell Telephone, is thus described: The "sender," or transmitter, is an adaptation of the microphone, and the results obtained are most satisfactory. The entire sending and receiving apparatus comes within the bulk of a lady's workbox of ordinary size, and in appearance the transmitter rather resembles that decorative piece of furniture. The sounding-board, upon which the message-sender must throw his voice, is the top panel of a neat cover which preserves the simple apparatus below from dust or risk of injury from chance knocks. The panel is of ordinary yellow pine wood, one-eighth of an inch thick, and nine inches long by five inches wide. On the under side of the panel are placed the carbon pencils, eight in number, ranged star shape from a central point, and electrically connected in separate series of four each.

In the receiver, which is below the carbons, the magnet is of peculiar shape, and of sufficient power to lift twenty times its own weight. It is formed of magnetized steel, and is of horseshoe shape, with the two ends turned inward and backward so as to form a diameter of the circle, but divided in the center. The two poles of the magnet are thus placed one before the other, as in Faraday's electro-magnet. The poles are tipped with iron which terminates in front in two thin iron plates, on which are placed the electromagnetic coils. The diaphragm is a thin iron plate, and is fixed firmly to the edges of a circular brass box, which forms a kind of sounding-box. The wire communication may run with other telegraph wires in the usual fashion, and it is claimed for the instrument that it works perfectly for distances up to thirty miles. The receivers are two tubes with small trumpet-shaped ends; they may be made of any reasonable length, and may be held up to the two ears, or may be laid across a room to different persons.

MOLECULAR ELECTRO-MAGNETIC INDUCTION.

Prof. D. E. Hughes, F.R.S., by no means content with having given to the world two of the most wonderful instruments in a century of wonderful instruments, is carrying out a new series of researches which seems destined to reveal many new wonders. The first of several papers on these researches was lately read before the Royal Society. In it Prof. Hughes stated that his induction currents balance had shown extreme sensitiveness to the slightest molecular change in the composition of metals and alloys, and gave evidence of peculiarities in iron and steel, for which their magnetic properties failed to account. The new investigation was commenced to obtain a cause for these peculiarities, and the experiments have been carried out by means of a new apparatus, in which the acting portion is the wire undergoing examination. The apparatus consists, first, of an instrument for producing the new induction current; second, of a sonometer or balancing coils; third, rheotome and battery; and fourth, the telephone. The diagram shows roughly the position of the apparatus, the wire under examination passing through the axis of the coil. The wire is rigidly fixed at one end, so arranged that torsion can easily be applied. The sonometer used is one founded on a principle laid down in *Comptes Rendus* in 1878. It consists of two coils at right angles, the internal coil movable about a vertical axis, and having a pointer clamped to it so as to enable the number of degrees of motion to be easily observed on a graduated scale. Whenever the axis of the interior coil is perpendicular to the exterior coil, no induction takes place, and we have a perfect zero; by turning the interior coil through any degree, we have a current proportional to this angle, and in the direction in which it is turned. As this instrument obeys all the well known laws for galvanometers, the readings and evaluations are easy and rapid. Prof. Hughes, in his paper, says, "If the coil upon the stress bridge is perpendicular to the iron wire, and if the sonometer coil is at zero, no currents or sounds in the telephone will be perceived, but the slightest current in the iron wire produced by torsion will at once be heard; and by moving the sonometer coil in a direction corresponding to the current, a new zero will be obtained, which will not only balance the force of the new current, but indicate its value. A perfect zero, however, will not be obtained with the powerful currents obtained by the torsion of two millimeters diameter iron wire. We then require special arrangements of the sonometer which are too complicated to describe here. The magnetic properties of iron, steel, nickel, and cobalt have been so searchingly investigated by ancient as well as modern scientific authors, that there seems little left to be known as regards its molar magnetism. Prof. Hughes uses the word molar here simply to distinguish or separate the idea of a magnetic bar of iron or steel magnetized longitudinally or transversely from the polarized molecules which are supposed to produce its external magnetic effect. Molar magnetism, while having the power of inducing an electric current in an adjacent wire, provided that either has motion or a change in its magnetic force, as shown by Faraday in 1832—has no power of inducing an electric current upon itself or its own molar constituent either by motion or change of its magnetic moment. Molecular magnetism has no, or a very feeble, power of inducing either magnetism or an electric current in an adjacent wire; but it possesses the remarkable power of strongly reacting upon its own molar wire, inducing—comparatively with its length—powerful electric currents in a circuit of which this forms a part.

After referring to the work in connection with the relation between stress and magnetism of Ampère, Matteucci, Westheim, Villari, Werdermann, and Sir W. Thomson, Prof. Hughes said from his own researches he was convinced that "we have, in molecular magnetism, a distinct and separate form of magnetism from that which we develop, or render evident, longitudinal or transversal magnetism, defined above as molar. If we place an iron wire, say 20 centimeters long, 1 millimeter diameter, in the axis of the coil of the electro-magnetic balance, and if this wire is joined to the telephone, we find that on passing an electric current through the inducing coil no current is perceptible upon the iron wire; but if we give a very slight twist to this wire at its free end—one-eighteenth of a turn, or 20 deg.—we at once hear, clear and comparatively loud, the currents passing the coil; and although we only gave a slight elastic twist of 20 deg. of a whole turn, and this spread over 20 centimeters in length, making an extremely small molar spiral, yet the effects are more powerful than if, using a wire free from stress, we turned the whole coil 40 deg. The current obtained when we turn the coil, as just mentioned, is secondary, and this with the coil at any angle, any current produced by its action, either on a copper, silver, iron, or steel wire. In fact, it is simply Faraday's discovery, but the current from an elastic



twist is no longer secondary under the same conditions, but tertiary, as I shall demonstrate later on. The current passing through the coil cannot induce a current upon a wire perpendicular to itself, but the molecules of the outside of the wire, being under a greater elastic stress than the wire itself, they are no longer perpendicular to the center of the wire, and consequently they react upon this wire as separate magnets would upon an adjacent wire. It might here be readily supposed that a wire having several twists, or a fixed molar twist of a given amount, would produce similar effects. It, however, does not, for in most cases the current obtained from the molar twists are in a contrary direction to that of the elastic torsion. Thus, if I place an iron wire under a right-handed elastic twist of 20 deg., I find a positive current of 50 deg. sonometer; but if I continue this twist so that the index makes one or several entire revolutions, thus giving a permanent molar twist of several turns, I find upon leaving the index free from any elastic torsion, that I have a permanent current of 10 deg., but it is no longer positive but negative, requiring that we should give an elastic torsion in the previous direction, in order to produce a positive current. Here a permanent elastic torsion of the molecules is set up in the contrary direction to its molar twist, and we have a negative current overpowering any positive current which should have been due to the twisted wire."

"The following table shows the influence of a permanent twist, and that the current obtained when the wire was freed from its elastic torsion was in opposition to that which should have been produced by the permanent twist. Thus, a well-softened iron wire, 1 millimeter in diameter, giving 60 deg. positive current for a right-handed elastic torsion of 20 deg., gave after 1 deg. 0·80 permanent torsion a negative current of 10 deg.

1	complete	permanent	torsion	(right-handed)	negative,	10
2	"	"	"	"	"	15
3	"	"	"	"	"	15
4	"	"	"	"	"	16
5	"	"	"	"	"	12
6	"	"	"	"	"	10
7	"	"	"	"	"	5
8	"	"	"	"	"	4
9	"	"	"	"	"	3
10		"	"	"	"	3

"At this point the fibers of a soft wire commence to separate, and we have no longer a complete single wire, but a helix of separate wires upon a central structure."

Sending the current through the wire with the telephone to coil the tertiary effects are obtained, but the effect is not found in using non-magnetic metals. It requires a great many permanent twists in a wire to be able to see any effect from these twists, but if we give to a wire, 1 millimeter diameter, forty whole turns—or until its fibers become separated—we find some new effects; we find a small current of 10 deg. in the same direction as its molar twist, and on giving a slight twist—20 deg.—the sonometric value of the sound obtained is 80 deg. instead of 50 deg., the real value of a similar untwisted wire; but its explanation will be found by twisting the wire in a contrary direction to its molar twist. We can now approach the zero, but never produce a current in the contrary direction, owing to the fact that by the spiral direction, due to the fibrous molar turns, the neutral position of its molecules are no longer parallel with its wire, but parallel with its molar twist; consequently an elastic strain in the latter case can only bring the molecules parallel with its wire, producing no current, and in the first case the angle at which the reaction takes place is greater than before, consequently the increased value of its current.

The measurements of electric force mentioned in this paper are all sonometric on an arbitrary scale. Their absolute value has not yet been obtained, as we do not, at our present stage, require any but comparative measures. Thus, if each wire is of 1 millimeter diameter, and 20 centimeters long, all render the same stress in the axis of its coil, and the following are the sonometric degrees of value:

Soft iron	60°	tertiary	current.
Hard drum iron	50	"	"
Soft steel	45	"	"
Hard tempered steel	10	"	"
Copper, silver, etc	0	"	"
Copper, helix, 1 centimeter diameter, 20 turns in 20 centimeters	45	secondary	currents.
Iron, spiral, ditto	45	"	"
Steel	45	"	"

The tertiary current increases with the diameter of the wire, the ratio of which has not yet been determined; thus, an ordinary hard iron wire of 1 mm. diameter, giving 50 deg., one of 2 mm. diameter gave 100 deg.; and the maximum of force obtained by any degree of torsion is at or near its limit of elasticity, as if in same time we also pass this point, producing a permanent twist, the current decreases as shown in the case of a permanent twist. Thus, the critical point of 1 mm. hard iron wire was 20 deg. of torsion, but in hard steel it was 45 deg.

Longitudinal strains do not produce any current whatever, but a very slight twist to a wire, under a longitudinal strain, produces its maximum effects; thus 20 deg. of torsion being the critical point of iron wire, the same wire, under longitudinal strain, required but from 10 deg. to 15 deg.

A large number of experiments have been made, some of which, through the kindness of Professor Hughes, we have witnessed, all tending to prove the theory he has advanced. He finds that heat has a very great effect upon molecular magnetic effects. On iron it increases the current, but diminishes it in steel. The paper we have thus briefly described was devoted to experimental facts by Prof. Hughes, who said:

"If we assume, with Poisson, that the paths of the molecules of iron are circles, and that they become ellipses by compression or strain, and also that they are capable of being polarized, it would sufficiently explain the new effects.

"Joule has shown that an iron bar is longer and narrower during magnetization than before, and in the case of the transverse strain the exterior portions of the wire are under a far greater strain than those near the center, and as the polarized ellipses are at an angle with the molecules of the central portions of the wire, its polarization reacts upon them, producing the comparatively strong electric currents described."

GRAY'S HARMONIC TELEGRAPH SYSTEM.

Mr. F. W. Cushing lately delivered an interesting lecture on Gray's Harmonic System before the New York Electrical Society, from which we take the following:

Before attempting a description of Gray's Harmonic Telegraph, which, as its name implies, has much to do with the phenomenon of sound, a few words on the simpler laws of acoustics will not be out of place.

If two bodies are brought suddenly together, every ear in the room receives a shock, to which the name of sound is given. The molecules composing the bodies are set into vibration, and the vibrations, acting upon the air with which they are in contact, produce air waves which travel out in all directions and finally reach the ear, producing upon the mind the sensation which we call sound. If a body is caused to vibrate, each vibration sends out an air wave, and a continuous sound, called a tone, is produced. When these vibrations are slow the waves are comparatively far apart, and the tone is a low one. When they are rapid the waves are closer together, and the tone is higher. Every body that can be made to vibrate has its fundamental tone; that is to say, it can move just so fast and no faster. The amplitude of its vibrations may be made greater, but this will only increase the volume of sound without altering the pitch. As an illustration, take a weight suspended by a piece of string. Move it from its position of rest an inch to the right; when it is released it will swing back to and, having received an impetus from the force of gravity, nearly an inch to the left of its original position. Then move it three inches to the right. When released it will swing nearly three inches to the left. Next time start it from a position a foot to the right, and it will swing nearly a foot to the left. Each of these swings of one, three, and twelve inches will occupy precisely the same time, and if the string were a solid body with one end fixed and the other vibrating with an amplitude of one, three, or twelve inches, the same number of air waves per second would be sent out in each case and the same note produced, but the volume of sound would be greater with the greater amplitudes.

These air waves are not only recognized by the human ear, but by any body having the same fundamental tone that they come in contact with and that is free to vibrate. Prof. Gray illustrates this by standing two tuning forks of the same tone near each other and causing one to vibrate for a few moments. Upon raising it, the other fork will be found to be vibrating also. The first wave strikes the nearest prong of the second fork and pushes it a little out of place, when its elasticity carries it back to and a little beyond its original position just in time to meet the second wave, when the former operation is repeated and the vibratory motion is sustained as long as the air wave lasts.

If, however, the forks have not the same fundamental tone, the second fork will not vibrate, because when the prong of the second fork is displaced by the first air wave and flies back, the second wave comes along either too soon or too late to take advantage of the motion communicated by its predecessor, and its effect is lost. Hence, in order to communicate vibrations from one reed to another by means of air waves, both must be tuned to the same note.

In transmitting vibrations through a wire by means of electricity, these general rules must be observed. At the sending end of the wire a reed is set into vibration, and each of its swings is made to send a wave of electricity over the wire. These waves, reaching the receiving end, pass around the cores of an ordinary electro-magnet, which has for an armature another reed with the same fundamental tone as the first one. Each pulsation of current magnetizes the soft iron core, which, in turn, attracts the reed and draws it out of place; then the current is broken, the core is demagnetized, and the reed, being set free, flies back to, and, on account of its elasticity, a little beyond its position of rest, when it is again attracted by another wave of current and the motion repeats itself as long as the current waves last. If the vibrator at the sending end be thrown in and out of circuit, the reed at the receiving end will start and stop exactly in accordance with it, and telegraphic signals may be transmitted, being received in the form of musical notes, a short note forming a dot and a long one a dash.

A very ingenious device has been invented by Prof. Gray to reduce these notes again into Morse characters upon an ordinary sounder. A small bar of metal, called a rider, is balanced upon a supporting piece, and has one end resting upon the receiving reed. A light adjusting spring is attached to the rider. One pole of a local circuit, containing a sounder, is attached to the reed and the other pole to the rider. When the reed vibrates the rider trembles upon it and makes the connection in the local circuit so poor that the sounder opens; the instant the reed stops vibrating, the adjusting screw pulls the rider firmly down upon it, restores the circuit, and the sounder closes. So that when the sending key is open, it being so arranged that the vibrator is then to line, the receiving reed is in motion and the receiving sounder is open; close the sending key, the vibrator is thrown out, the receiving reed becomes quiet, and the sounder closes, producing the same effect as in sending over a single wire with ordinary Morse apparatus. It may seem incredible that all these arrangements should work so nicely together as to allow of rapid transmission, but at a recent test of five hours between New York and Boston, 376 messages were actually sent over one tone, being an average of over 75 per hour, and probably the fastest time ever made on a Morse circuit where ordinary business was handled.

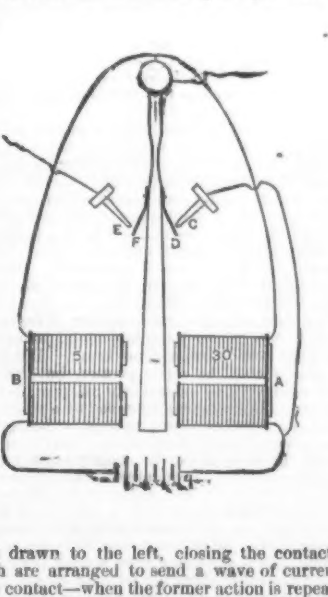
As operated at present, four vibrators with different fundamental tones are placed at the sending end, and four receivers, so tuned that each will equal one of the vibrators in tone at the receiving end of the wire, and the four series of vibrations are transmitted simultaneously, each receiver responding only to its own sender. Thus, we have four messages going in the same direction over one wire, and at the same time. In order to break, resistance is thrown in and out by the opening and closing of a key at the receiving end, which throws a relay that has been adjusted over the tone current at the opposite end out of adjustment, and records the signals. All the receiving operators use the same break key without confusion.

Prof. Gray has transmitted as many as eight tones at once, but the margin between them was so small and such very delicate adjustment was necessary, that, for practical work, he adopted four tones only, and so developed the principle into the present harmonic system.

The following diagrams and explanations will assist the reader in more thoroughly understanding the principle of the harmonic system:

THE VIBRATOR.

The electro-magnets, A and B, have coils of 30 and 5 ohms of resistance, respectively. When the current leaves + pole of the battery and magnetizes A, the reed is drawn to the right and closes the contact points, C D. The current is then shunted around A, increasing the power of B, and

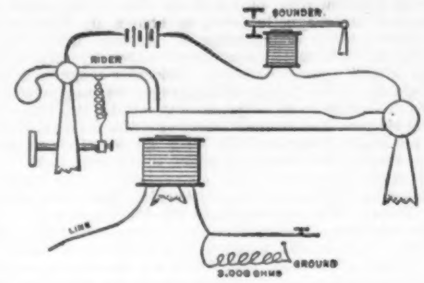


the reed is drawn to the left, closing the contact points, E F—which are arranged to send a wave of current to the line at each contact—when the former action is repeated, and the reed is in a state of vibration. The speed of the vibrations is governed by the fundamental of the reed.

RECEIVING APPARATUS.

When the reed in the receiving relay is in a state of vibration, caused by the action of the incoming waves of current, the local circuit, in which is included the reed and rider, becomes so imperfect (caused by the rider's trembling) that the sounder opens. When the reed returns to a state of rest the contact becomes perfect and the sounder closes.

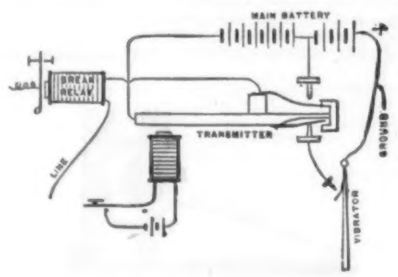
The break key, when not in use, is left open, forcing the current to travel through about 3,000 ohms of resistance (or more, according to the length of the line) to find a ground. When the key is depressed, the current takes a new route of no resistance to ground, and the current is sufficiently increased, by having so much less resistance to encounter, that the magnet of the break relay, at the sending station, overcomes the tension of its armature spring and



closes, recording the signals made upon the key at the receiving station.

SENDING APPARATUS.

When the reed swings to the left the battery is short-circuited through the transmitter lever, lower spring, and contact points. When to the right the metallic circuit is broken and + pole finds ground at the home, and — pole through the line at the distant station. Instead of actually opening and closing the battery, the action of the vibrator only reduces its strength about 60 per cent., and, as it is necessary that the same amount of current should always be to line to



allow of the break relays being adjusted over it, the points are so arranged that when the transmitter is closed, cutting off the vibrator, the upper spring and point come into contact and throw about 40 per cent. of steady current to line. So that, whether the key be open or closed, the same battery strength is always going to line. When the key is open it is being sent in pulsations (too close together to affect a Morse relay), and when the key is closed it is being sent steadily.—*Operator.*

MAGNETO-ELECTRO INDUCTION.

By F. GUTHRIE and C. V. BOYS.

A CONDUCTOR in a moving magnetic field is urged to move by a force varying as the product of the conductivity into the relative speed, so that by observing the torsion of a wire supporting successively different substances of the same form and size in a revolving magnetic field, a measure of their relative conductivity may be obtained. This method seems specially applicable to electro lights, owing to the absence of electrodes, electrolysis, and polarization. The apparatus employed consisted of a glass shade containing one liter of liquid suspended by ebonite strips to a horizontal boxwood beam, which was hung to a long thin steel wire. Completely surrounding the vertical circular sides of the vessel is a powerful magnet, consisting of twenty-four semicircular bars; a remarkably uniform field of force through the liquid is produced. The magnet is fixed to the top of a vertical steel shaft beneath the vessel, and the shaft (and with it the magnet) is made to revolve with great rapidity by a band driven by steam power. To protect the glass vessel, etc., from the whirlwind caused by the revolving magnet, a screen is interposed between them and the magnet. The speed of the latter is measured by the number of turns per second as indicated by a wheel turning once for 10,000 of the magnet, and a bell striking at every one hundredth turn. When the magnet is revolving rapidly, say three thousand turns per minute, the liquid experiences a force tending to turn it with the vessel in the same direction, but the vessel comes to rest from the torsion of the wire, and the motion of the liquid is checked by the friction between its cylindrical layers, and between it and the glass, so that its actual revolving motion is very slow, giving rise to no appreciable error (only 1 in 20,000), and the friction is balanced by the torsion of the suspending wire, so that the deflection of the vessel is a measure of the force between the magnet and the liquid, and therefore of its conductivity. The deflection is observed by a microscope directed toward a scale fixed to the vessel.

The practical difficulties appear to have been considerable, and arose mainly from the variation of steam pressure and from the impossibility of finding the zero of the scale, owing to the position of equilibrium of the vessel being different for different positions of the magnet when at rest. It was, therefore, necessary to make observations of the deflections at different speeds, together with an observation when a disk of brass was suspended from the boxwood beam, and combine them so as to eliminate the unknown factors. The only substances experimented on were sulphuric acid and sulphate of copper, the results for which are given. The conductivity curve for the former agreed substantially with Kohlrausch's, who used alternating currents. There is a sharper rise to and fall from the first maximum, otherwise the position of the two maxima, of the minimum, and of the point of contrary flexure agreed most perfectly.—*Phil. Magazine.*

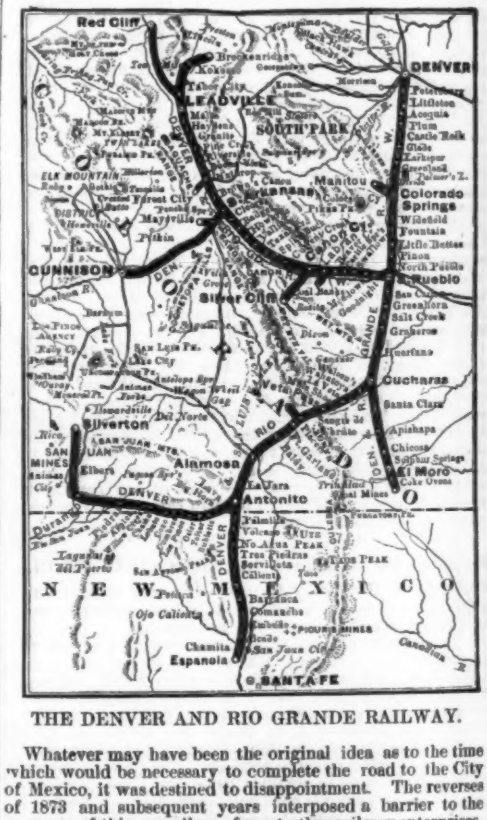
THE BEEF JUICE FUROR.

IN the present *furor* for fluid beef juice, says Dr. Fothergill, the necessity for starchy matters is being quite overlooked, or, to be very safe, underestimated. These meat products furnish—the best of them—little glycogen or animal starch, and yet that is the fuel food of the body *par excellence.* We must be guided by rational knowledge, by physiology and not by fashion, in our dietetics. When there is very feeble digestion, then the digested milk and milk gruel advocated by Dr. Roberts is to be employed.—*The Practitioner.*

THE DENVER AND RIO GRANDE RAILWAY.

THE rapidity with which the Denver and Rio Grande Railway has spread its iron network over a large portion of Colorado and far into New Mexico shows wonderful energy and indomitable pluck on the part of its managers. No more difficult field for railway construction was ever offered to the engineer. The map gives no idea of the character of the country, nor can one fully appreciate the obstacles to railway construction until he has traversed its gorges, cañons, cliffs, and passes. The lines which of necessity look so straight and direct in the map, are in reality exceedingly tortuous. The broken, rugged, mountainous character of the country renders it necessary for the road to wind, twist, climb, and bore its way—often going many miles to accomplish a distance which in an air line would be but a small fraction of a mile. *A priori* it seemed impossible that the locomotive could ever climb such giddy heights and snort its defiance on the brink of such awful precipices. Much less did it seem possible that the work could be achieved so quickly and so well. But doubt has changed to wonder, and wonder to admiration. Henceforth the Colorado tourist will hesitate long before pronouncing any feat impracticable for the builders of railways. The little road which erstwhile extended along the mountains' base from Denver to Pueblo was hardly thought to have such inherent power. It has developed into a veritable "little giant," and has really but just begun its growth. As the infant Hercules in his cradle strangled the serpents, so it has already conquered innumerable obstacles, and set out upon a career of labors and conquest.

The Denver and Rio Grande Railway Company was incorporated October 27, 1870, for the purpose of building a road from Denver to El Paso on the borders of old Mexico, and thence, if suitable concessions could be obtained, to the City of Mexico. It was the first narrow gauge railway of any considerable size, and has kept in the van, being now the largest narrow gauge in the world. The first American-built locomotive was placed on its track in the summer of 1871. The road was opened from Denver to Pueblo, 120 miles, June 15, 1872; and a branch to the Cañon Coal Mines, 43·9 miles, was completed October 16th of the same year.



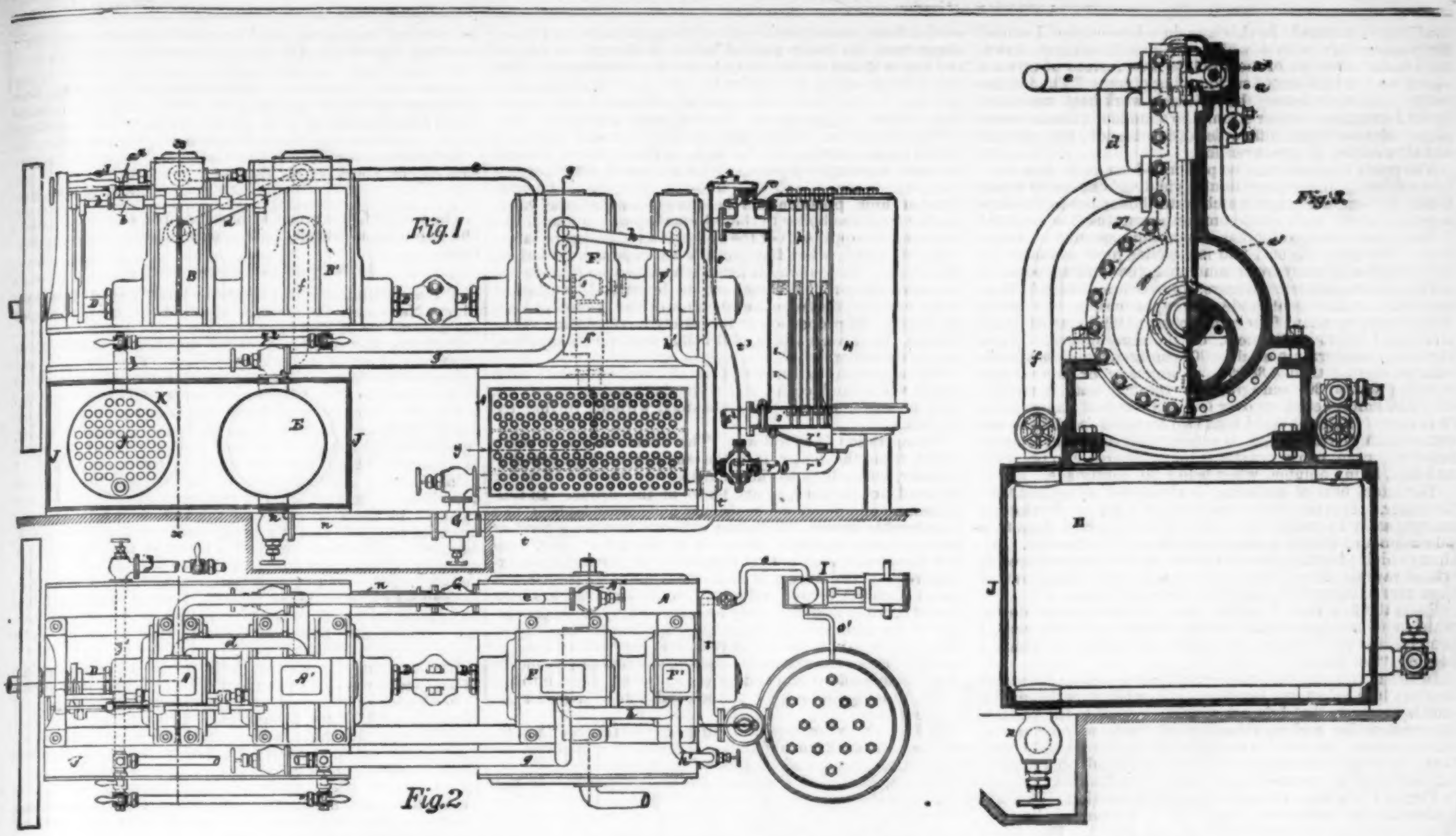
THE DENVER AND RIO GRANDE RAILWAY.

Whatever may have been the original idea as to the time which would be necessary to complete the road to the City of Mexico, it was destined to disappointment. The reverses of 1873 and subsequent years interposed a barrier to the progress of this as well as of most other railway enterprises. In 1876 the road was completed to El Moro and to La Veta. The latter branch was built to Fort Garland in 1877, and to Alamosa June 15, 1878. From December 14, 1878, to December 31, 1879, the company was under a receivership, and the road was operated by the Atchison, Topeka and Santa Fe under a lease. The details of the struggle for the right of way through the Grand Cañon of the Arkansas, and for the possession of the road are still fresh in the minds of our readers. The receivership and the alleged lease terminated March 27, 1880, and the courts gave possession of the Grand Cañon to the Denver and Rio Grande Railway.

The revival of railway construction had at that time assumed great proportions, and this company at once adopted a policy of vigorous extension in every direction which promised to pay. Mining development was at its height, and inducements for railway construction were held out on all sides. On December 31, 1879, the company had in operation 339½ miles of road. There are now in operation, according to the published time-tables, the following lines:

Denver to Leadville, via Pueblo........	280 miles.
Pueblo to Espanola (N. M.)........	250 "
Cucharas to El Moro........................	36 "
Colorado Springs to Manitou..................	6 "
Nathrop to Alpine..........................	14 "
Total......................................	586 "

Many more miles have, however, been completed and will appear in subsequent time-tables. Extension has progressed even during the severe winter weather, and it is hardly possible to state just how much track has been laid. Up to the beginning of the present month, the following additions to the above mileage had been completed. From Leadville to Robinson Camp, 16½ miles; to Malta, near Tennessee Pass, 10 miles; South Arkansas to Maysville, 12 miles; Poncho Springs (6 miles from South Arkansas) to Silver Creek, 8 miles; from Cañon City toward Silver Cliff, 12



THE GAMGEE PERPETUAL MOTION OR THERMO-DYNAMIC ENGINE.

miles; from Antonito toward Durango and Silverton, 78 miles; to stone quarries and coal mines 4 miles. This gives a total of about 726 miles now completed. The work on all the extensions is to be pushed rapidly as soon as spring opens; and several lines have been decided upon. Plans of extension are influenced by changing circumstances, and it is not announced just what will be done this year, except that it is intended to build about 500 miles of road. Durango, Silverton, Silver Cliff, Gunnison, Red Cliff, and Breckinridge will soon hear the whistle of construction trains; and the tourist or business man can reach these points by rail this summer.

The construction of the road has by no means been carelessly done. It is built in the best manner, steel rails being used on all the new lines. Much of the old track has also been relaid with steel, and there is now less than 200 miles of iron rail, which is rapidly being replaced by steel. The equipment is also first class, the motive power being admirably adapted for its work. Horton chair cars for day use and Pullman sleeping cars are run on the principal lines of travel. The business of last year was very large, and the prospects for the coming year excellent. Tourist travel is already very great, and will increase immensely as the attractions of this scenic paradise become more widely known. A region more fascinating to the tourist, the hunter, the artist, or the seeker after health, than that made accessible by this road, does not exist.—*Railway Review.*

PROF. GAMGEE'S NEW MOTOR—THE AMMONIA ENGINE.

CONSIDERABLE attention has lately been directed to this invention, more especially because according to the specification, it appears to be a self-propelling mechanism, or perpetual motion; moreover it is alleged to have received the favorable indorsement of the Patent Office Examiner and of Chief Engineer Isherwood of the United States Navy. We give herewith the specification and drawings of the patent in full:

Thermo-dynamic Engine. Letters Patent Nos. 240,400, dated April 19, 1881. Application filed February 26, 1881. (No model.)

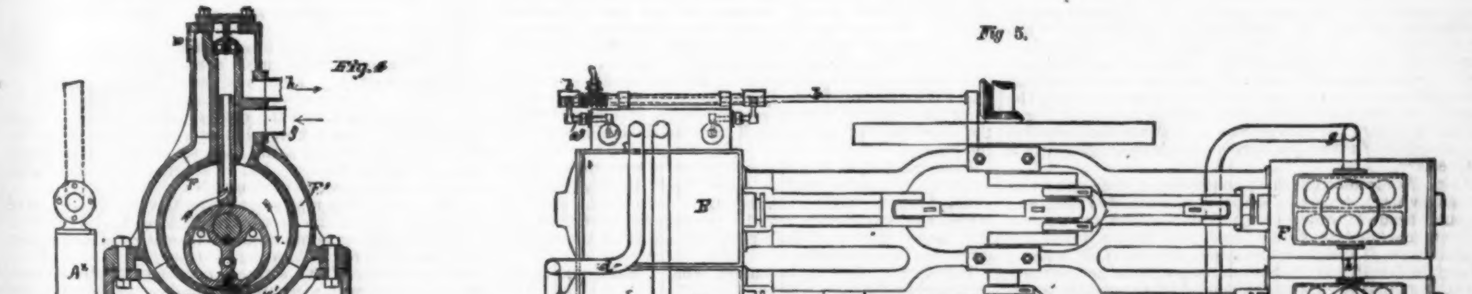
Be it known that I, John Gamgee, of London, England, at present residing in the city of Washington, District of Columbia, have invented certain new and useful improvements in thermo-dynamic engines, or, more particularly stated, in the method of, and apparatus for, using a liquefiable gas or vapor at low temperature as a motor fluid, of which the following is a specification:

My invention relates to the employment, as a motor fluid, of a liquefiable gas or vapor of adequate tension, the product of a liquid which boils at or near the temperature of surrounding objects. I find that by working such a gas or vapor expansively in one or more engine-cylinders its heat can be converted to such an extent into mechanical energy or motion, that at the exhaust it will have returned, in great measure, to its original liquid condition, from which state it may be again caused to assume the condition of a motor vapor or gas, by exposing it to the needed temperature. It is this feature, viz., the working of such a vapor or gas expansively to the extent of more or less complete liquefaction, and then reconverting it from the liquid to the vaporous or gaseous condition for use again as a motor fluid, which mainly characterizes my invention.

The vapor having expended its energy and being mostly liquefied by the conversion of its heat into motion, is discharged from the engine-cylinder into a closed exhaust-vessel protected or insulated from environing heat. The maintenance of the exhaust at the boiling point (for atmospheric pressure or thereabout) of the liquid used may be insured in various ways—for instance, by means of an injector or pump. By the injector or pump, or both, the cooled vapor is forced into an apparatus, for convenience sake, termed a "boiler," where it is exposed to the temperature needed to restore it to its original tension, and thence returns to the engine.

It will thus be seen that it is my object to obtain in a motor-engine the conditions of a closed circuit with a liquid boiling at a low temperature relatively to water transformed into vapor, the molecular energy of which is converted into the mass or molar motion of the piston, so that its initial

Fig 5.



THE GAMGEE PERPETUAL MOTION OR THERMO-DYNAMIC ENGINE.

condition is restored. In this way, in a heat-engine, I extend the temperature within which the heat is utilized downward in the direction of the absolute zero, instead of upward above the temperature of surrounding objects. The intense heat of boiler furnaces, the internal work-heat necessary to the formation of water steam, the abundant exhaust waste of the steam engine, difficulties of lubrication, etc., are one and all avoided by my invention.

The cycle I propose can be performed more or less satisfactorily with almost any liquid yielding expansive vapor below the temperature at which water boils; but in developing most power with most compact apparatus it is essential to use a compound which has a maximum amount of latent heat. The agent which I find in practice most available for this purpose is anhydrous ammonia, the boiling point of which, at atmospheric pressure, approaches closely to 34·4° centigrade. At 0° centigrade its vapor tension is 3,183·34 millimeters, or about four atmospheres, while at 10° it attains to 4,574·03 millimeters, or six atmospheres. When the mean temperature attains 20° centigrade no less a pressure is exerted than 6,387·78 millimeters; or nine atmospheres; and at 30° centigrade, or tropical heat, it reaches over 8,000 millimeters, or over ten and one-half atmospheres in tension. Since at blood heat two hundred pounds to the square inch is available, it is evident that the usual temperature of ocean or river water is most desirable in practice, and best, in my opinion, when below 20° centigrade.

The latent heat of ammonia is about 900° as against 960° for water. It is this latent heat which I use in developing energy, so as to reduce the amount of rejected heat to a minimum and obtain a maximum rate of liquefaction. Although high pressures are attainable at low temperatures, it will always be found best in practice to work below rather than over one hundred pounds to the square inch.

From the fact that I utilize heat in this system downward to 0° centigrade and below toward absolute zero, I propose, for convenience, to name the apparatus which I employ "zeromotor."

In the accompanying drawings is represented an apparatus to carry into effect my invention I wish it to be understood, however, that I do not restrict myself to the particular construction and combination of parts which compose the apparatus, for these may be varied to suit special conditions, so long as the apparatus as a whole is adapted to carry out the cycle of operations hereinbefore specified.

Figure 1 is a side elevation, partly in section. Fig. 2 is a plan of the apparatus. Fig. 3 is a section on line $x\,x$, Fig. 1. Fig. 4 is a section on line $y\,y$, Fig. 1. Fig. 5 is a view of a modification hereinafter referred to.

The engine shown in the drawings is a double-cylinder rotary engine, B being the first or high-pressure cylinder, and B' the second or low-pressure cylinder, into which the first cylinder exhausts through pipe, *d*. As seen in section, Fig. 3, the gas or vapor enters the cylinder, B, through the valve, *a*, and sliding division-port, *a'*, which runs in contact with the eccentric rotary piston, C, in the usual way. The admission-valve is operated from the rocking valve-rod, a^3, in the usual way, said rod having an arm, a^4, which bears against the rotating adjustable cut-off cam, *b*, whose shaft, *b'*, is rotated through the medium of eccentrics and connecting-rods from the main shaft, D, in the ordinary manner. The exhaust-port of the first cylinder is shown at d', in communication with the exhaust-pipe, *d*, which leads to the gas or vapor admitting valve of the second cylinder, B'. The latter, with its accessories, is similar, except in size, to the first cylinder, B, the shaft D being common to both, and the two cylinders are combined together for operation in the same way as the two cylinders of an ordinary compound or double-cylinder rotary engine.

The pipe, *e*, conducts the liquefiable gas or vapor to the primary cylinder, B, from the dome, A^x, of the part A, which, for convenience' sake, will be termed the "boiler," and which will be hereinafter described.

The exhaust-pipe, *f*, from the second cylinder, B', leads into the closed exhaust-vessel, E. This vessel receives, through the exhaust-pipe, *f*, the liquefied vapor and gas from the second cylinder.

From the upper part of the exhaust-vessel leads a pipe, *g*, to the larger cylinder, F, of a compound or double cylinder rotary pump, F F', driven by the rotary shaft, D. The larger of these pump-cylinders is shown in section in Fig. 4. The smaller or high-pressure cylinder, F', with its accessories is the counterpart of the other, and has its induction-port in communication with the eduction-port of the larger cylinder through the intermediary of pipe, *h*. The eduction-port of the smaller cylinder, F', communicates, through pipe, *h'*, with the space in boiler, A, which receives the liquid from which the motor gas or vapor is to be generated. The pump being of known description, and the arrows in Fig. 4 indicating plainly the direction of motion and the induction and eduction, further description of this instrumentality is unnecessary.

Boiler, A, is a metallic shell, containing at each end an inner hollow head, A', and a space, *i*, between that head and the outer head, A, to receive the liquid—preferably water—which takes the place of fuel as a heater for the liquid ammonia. Pipes, *j*, lead from one space, *i*, to the other, passing through without having communication with the hollow inner heads, A'. Into the space, *i*, on one side enters the water-induction-pipe, *i'*, and from the opposite space, *i*, leads a water-eduction-pipe, i^2. Water entering through pipe, *i'*, will circulate through spaces, *i*, and pipes, *j*, and will pass out through i^2. The interiors, *k*, of the hollow heads, A', are in communication by means of pipes, *l*, each of which surrounds concentrically one of the pipes, *j*, and is sufficiently larger to leave between it and the pipe, *j*, an annular space through which the ammonia can pass from one space, *k*, to the other. The water is permitted by proper means to circulate, not only through pipes, *j*, but also around the exterior of the ammonia-pipes, *s*. Alternating partitions, *m*, are formed in the spaces, *k*, so that the ammonia-vapor shall be caused to follow a tortuous path in passing through the pipes, *l*, and spaces, *k*.

The eduction-pipe, *h'*, from the pump leads into the lower part of one of the spaces, *k*, and through this pipe the pump discharges into the ammonia-space of the boiler, A, any ammonia vapor or gas that may be in the exhaust vessel, E.

From the bottom of the exhaust-vessel leads a pipe, *n*, which communicates with the lower part of the other one of the spaces, *k*, and through this pipe is conducted from the exhaust into the boiler the ammonia which has liquefied by reason of the conversion of its heat into mechanical energy. In order to force the liquid into the boiler I make use of an injector (indicated at G), using as motive power therein a jet of ammonia gas or vapor at high pressure, obtained from an auxiliary ammonia-boiler, H. This boiler is supplied with ammonia by means of a small steam or other power-driven pump, I, which draws the supply of ammonia needed from some suitable part of the apparatus—in this instance from the lower part of boiler, A, through a pipe, *o*, and forces it into the auxiliary boiler through pipe, *o'*. The boiler, H, is stayed by bolts or tie-rods, *p*, to resist pressure, and heat is supplied to the ammonia it contains by means of a system of piping, *r s*. Heated water, supplied from any suitable source, enters the space, *r'*, through pipes, r^2, thence passes out through the pipes, *r*, thence down through the outer concentric pipes, *s*, which are closed at their upper ends, into space, *s'*, and out through pipe, s^2. Ammonia-vapor at high pressure can be thus generated, the vapor passing from the boiler to the injector through a pipe, *t*. In the pipe, *t*, or upper part of the boiler, is a regulating or safety valve, *t'*, which, when the pressure exceeds the prescribed limit, rises. In so doing it lifts a pivoted lever, t^2, attached to a connecting-rod, t^3, which, at its lower end, is attached to the valve, *r*, that regulates the flow of the incoming heating-water. In proportion as the safety or regulating valve, *t'*, rises, the valve, *v*, closes, and I thus regulate the supply of heat to the boiler.

The apparatus is provided at all needed points with proper valves and cocks, and with check-valves to prevent back pressure, as will be understood without further explanation.

The operation is as follows: The ammonia gas or vapor passes from the boiler into the smaller or high-pressure cylinder, where it is worked expansively, the cut-off being adjusted, for instance, to one-tenth of the stroke. In thus expanding and doing work, the gas parts with its heat to a considerable extent. It thence exhausts into the second or lower-pressure cylinder, where it is cut off at, say, one-half the stroke, and is thus caused to do further work expansively. The result is that the vapor, by the time it passes from the second cylinder into the exhaust, has been almost entirely liquefied, only an exceedingly small proportion of the ammonia retaining vaporous form. The engine thus may be said to act not only as a motor, but as the condenser. From the exhaust vessel the ammonia is, by means of the compound pump and injector, forced back into the boiler, to be again brought to the condition of a motor gas or vapor.

In Fig. 5 is represented, in further illustration of my invention, a plan of an apparatus in which a compound reciprocating engine and compound reciprocating pump are employed, instead of the rotary engine and pump. In this apparatus the high and low pressure cylinders are horizontal, and in each cylinder the exhaust on each side of the reciprocating piston leads from the lowest part or bottom of the cylinder, so that the liquefied ammonia can be conducted off without difficulty. The lettered parts in this figure correspond in function to like lettered parts in the preceding figures. The compound engine and pump require no detailed description, being constructed and arranged in a manner similar to compound reciprocating steam-engines and pumps which have heretofore been used.

In order to keep the pump cylinders cool, they are preferably surrounded by a water-jacket, as indicated at F^2, Fig. 4. The cooling liquid enters at *w*, and is discharged at one or the other of the openings, *w'*.

In order to shield the exhaust vessel from the heat of surrounding objects, I inclose it in a metallic tank, J. The confined air within vessel, J, forms a good non-conductor of heat.

It will ound desirable, in many cases, to have one or more ves accessory to the exhaust vessel, in which a vacuum may b maintained or absorbents held, for the purpose of relieving the exhaust vessel or of emptying any part of the machine, as circumstances may require. Such a vessel, which may be called an "absorber," is shown in Fig. 1, at K, placed in the same tank, J, which contains the exhaust vessel. It is constructed, for the most part, like a tubular boiler, the tubes, *y*, being intended for the circulation of warm water or other heating medium. A pipe, *z*, leading from the ammonia space of the boiler or exhaust or other suitable part of the apparatus, enters the vessel, and is perforated, as shown at *z'*, to permit ammonia to pass therefrom into the vessel, which is to be filled with some absorbent of ammonia. This vessel is intended to serve as a place where the ammonia can be stored temporarily, when access is to be had to any part of the interior of the apparatus.

By properly regulating the cocks and valves with which the apparatus is provided, the ammonia can be diverted into this vessel, where it will be taken up and held by the absorbent. Whenever it is desired to withdraw the ammonia therefrom, warm water or other heating medium is caused to circulate through the tubes or heating space of the vessel. This causes the vaporization of the ammonia, and said vapor is carried off by the pump through the valve or cock controlled pipe, z^2, which communicates at one end with the upper part of the vessel, K, and at the other end with the pipe, *g*.

I remark that, in lieu of the injector, a force pump, such as pump, I, can be used to force liquid directly from the exhaust into the boiler. In fact, there are various means by which the equilibrium may be permanently disturbed, so that the exhaust may, during the operation of the engine, discharge into the boiler.

Having described my invention, what I claim and desire to secure by letters patent is—

1. The method of condensing a liquefiable gas or vapor (the product of a liquid of low boiling point) used as a motor fluid in a thermo-dynamic engine, which consists in working said gas or vapor expansively to the extent of more or less complete liquefaction in giving motion to the engine, substantially as hereinbefore set forth.

2. The method herein described of using a liquefiable gas or vapor (the product of a liquid of low boiling point) as a motor fluid for engines, which consists in working said vapor or gas in the engine expansively to the extent of more or less complete liquefaction, then exhausting the vapor thus liquefied into a suitable receiver, thence conveying it to a boiler, where it is subjected to the low degree of heat needed to bring it again to the condition of a motor gas or vapor, and thence returning it to the engine, to again go through the same cycle of operations, substantially as hereinbefore set forth.

3. The combination of an engine proper, in which a liquefiable gas or vapor is worked expansively to the extent of liquefaction, so that said engine shall serve not only as motor, but as condenser, a closed exhaust vessel, which receives the liquefied gas or vapor from the engine cylinder, a boiler, and means, substantially as described, for forcing the contents of said exhaust vessel directly to the boiler, the combination being and acting substantially as hereinbefore set forth.

4. In a thermo-dynamic engine, in which a liquefiable gas is used as the motor fluid, substantially as specified, the combination with the engine cylinder of a closed liquefied gas receiver or exhaust vessel protected by a non-conducting covering from the heat of the environment.

5. In a thermo-dynamic engine, vessels accessory to the exhaust vessel in which a vacuum may be maintained or absorbents held, for the purpose of relieving the exhaust vessel at any moment; or emptying any part of the machine, as circumstances may demand, substantially as set forth.

In testimony whereof I have hereunto set my hand this 25th day of February, A. D. 1881.

JOHN GAMGEE.

The Patent Office seems to have granted this patent without requiring a model or the production of a working machine.

PROFESSOR NEWCOMB'S OPINION.

A correspondent of the *Tribune* lately asked Professor Simon Newcomb, the eminent physicist, for his opinion of the new device of Professor Gamgee

Professor Newcomb said: "The question is purely one of physics, and not of steam engineering. The proposed machine, as Mr. Gamgee has explained it to me, and as I see it described in Mr. Isherwood's report, lacks the essential conditions which all experience shows a steam-engine must fulfill; not merely because ammonia is used instead of steam, but because no source of external cold or exit for the vapor is employed, except that furnished by the engine itself. I think there is some mistake in describing the respective functions of the high and low pressure boilers in the printed remarks in the *Tribune;* but I think I see clearly what the essential principle is. We have a boiler of liquid ammonia exerting an enormous pressure at ordinary temperatures. A quantity of the vapor from this boiler is admitted into the cylinder of the engine, and thus presses upon the piston, expanding and moving the piston. Its heat is changed into force communicated to the piston, and it thus becomes in the cylinder intensely cold, so cold that a portion of it liquefies.

"So far there is no trouble in the action of the engine. It will make one stroke without doubt. The question now is to dispose of this cool and expanded vapor. The great mistake made by the promoters is in supposing that they can, by some ingeniously contrived machinery, force the vapor back again, so as to act again on the engine and still have a surplus of force left over. It is a perfectly established law of gases—as certain and universal as that of gravitation—that a gas, when condensed, generates the same amount of heat and exerts the same pressure as in expanding. The consequence is that, when the gas is condensed without some external source of cold, all the power expended in its expansion is used up again in contracting and heating it. Unless, therefore, as in the ordinary steam-engine, some external source of cold is provided to absorb the heat which would thus be generated, the machine cannot act. Now, this is the very condition which Mr. Gamgee proposes to dispense with. With the ammonia engine working at ordinary temperatures, the external source of cold must be as low in temperature as the expended ammonia itself, and therefore the ammonia cannot be used for the cold.

"To judge of all this, we must remember that there is absolutely no new principle claimed in connection with the machinery, and claims made for it are in direct contradiction to the second law of thermo-dynamics. Yet I do not think a prudent physicist would claim that it was impossible to find in nature some mechanism by which this law could be evaded. All we can say is that to reach this result some radically new discoveries in the properties of matter must be applied. As there is nothing new in any of the principles called into play in the proposed engine, it may be pronounced a chimera with as much safety and certainty as we call perpetual motion machines by that name."

In reply to Professor Newcomb, Professor Gamgee wrote as follows:

THE GAMGEE ZEROMOTOR.

To the Editor of the Tribune :

SIR: The great weight attaching to any utterance of Professor Simon Newcomb on questions of physics induces me to express regret that he has not witnessed experiments, continued for nearly four months, in the Washington Navy Yard. The results obtained are absolutely conclusive in refutation of the position maintained by the Professor in yesterday's *Tribune.*

I do not appreciate the nice distinction expressed as follows: "The question is purely one of physics and not of steam engineering." I am not aware of any question in relation to the theory of the steam engine not purely physical. The physicist and the engineer base their knowledge on experiment, and nothing but experiment establishes the physical or engineering truth. It has been my good fortune, acting not in violation of, but in strictest accordance with thermo-dynamic law, to demonstrate:

First—That low temperature engines, receiving heat from their environment, can be worked utilizing heat downward to near the boiling point of such an agent as ammonia, and far below the zero of Fahrenheit's scale.

Second—That Watt's separate condenser, with an external cooling agent or discharge of the heat carrier or motor fluid into the atmosphere, essential in the steam engine, can be dispensed with in a zeromotor.

Third—That cylinder condensation and refrigeration produced by the expansion of a liquefiable gas like ammonia, against a working piston, are so abundant and constant, under right conditions, as to afford us an ample margin of available power, even after pumping to the boiler the residual vapor and liquid.

Fourth—That we are not acting in violation of "a perfectly established law of gases—as certain and universal as that of gravitation—that a gas when condensed generates the same amount of heat and exerts the same pressure as in expanding." We are not required to reload, except in small part, with heat, the ammonia issuing from the exhaust.

When, on the 20th of last December, I first started the refrigerating machine, which so completely bore out my anticipations, and the report of a competent board of naval engineers, I watched with anxiety an alcohol thermometer on the exhaust side of the ammonia engine. The temperature fell rapidly to 8° Fahr. and then slowly to 1°, when a weak solution of chloride of magnesium, surrounding the tubes of my refrigerator, became solid and we had to stop. The steam engine alone was powerless to move the machine, and the ammonia engines then saved us eighteen indicated horse power on a total required of between fifty and sixty horse. Since that day the verification of the first observations have been uniformly satisfactory, and temperature of the exhaust between 10° and 20° below zero Fahrenheit, have been readily maintained without extraneous refrigeration and with absolute constancy.

The ammonia, gaseous on entering the first cylinder, was so freely liquefied in doing work that while the engines became covered with a thick layer of frost the liquid appeared in abundance in the gauge glass.

Such condensation did not require power, but resulted from the transformation of the heat in the ammonia into the motion of the engine pistons.

For the first time, and in accordance with thermo-dynamic law, "a cold condenser was obtained cheaper than a hot boiler," without a colder surrounding. These words I borrow from an intelligent mechanic, himself an inventor, who grasped the problem so soon as presented to him.

There is so much new in the machinery I use that Professor Newcomb, had he seen it, could never have stated, while impeaching its novelty, that our plan was laid "in direct contradiction to the second law of thermo-dynamics." Would this, if true, not be new and absurd?

I do not pretend to have found "in nature some mechanism by which this law could be evaded." The "radically new discoveries in the properties of matter," essential in Professor Newcomb's opinion to upset nature's cycle, are not only unnecessary, but the point I perceived and which has to this day been overlooked is that a Plutarchian "primum frigidum" is not the indispensable concomitant of energy in heat engines. The calculations of Clausius and Maquorn Rankine, confirmed by the experimental results obtained by Isherwood and Hirn, guided my path in a work suggested as much, perhaps, by my knowledge of physiology as of engineering.

I hesitate to prolong a technical discussion in your columns, and conclude with an earnest invitation to Professor Newcomb to watch our experiments. In a few weeks all will be ready and the engine will make, not "one," but the first of countless continuous revolutions and establish the soundness of the principles involved in the zeromotor. I remain, sir, your obedient servant. JOHN GAMGEE.

Washington, D. C., April 29, 1881.

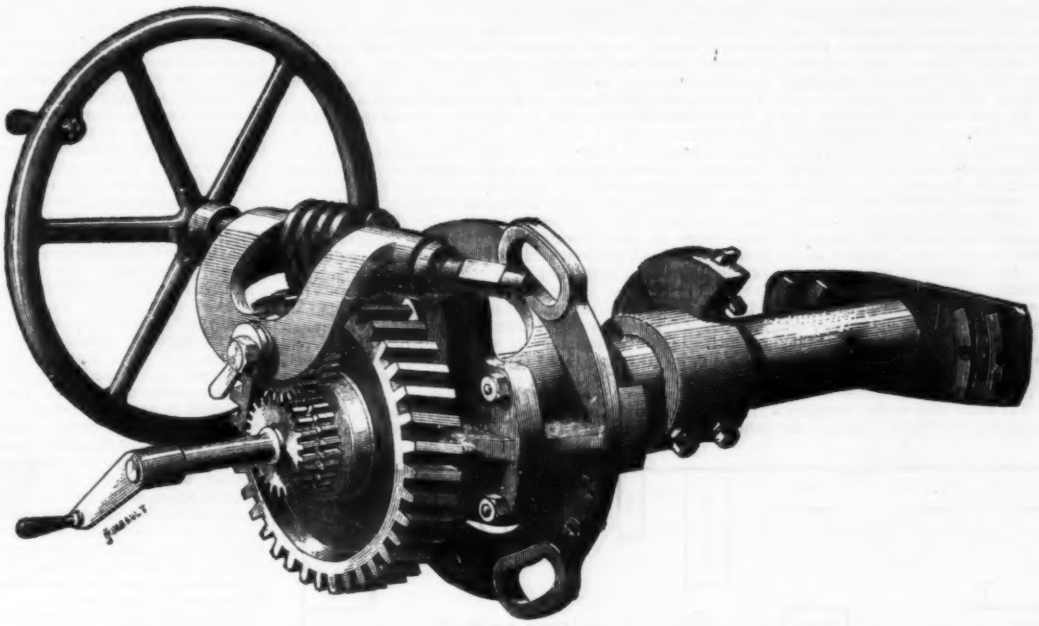
This letter is of interest chiefly as presenting a reiteration of the allegations of the patent, to wit, that the invention is a self-motor or "perpetual motion;" and further, as showing that the lunacy of the author is shared by prominent officials at the Navy Yard in Washington. It would seem that the "experiments" that led to this foolish patent were probably made at the public expense. Curiously enough this new machine that is to drive itself and also "afford us an ample margin of available power" is, like the Keely motor, and other perpetual motors, not quite finished yet, but "in a few weeks all will be ready;" and then it will run for ever, or until she wears out.

MACHINES FOR REPAIRING LOCOMOTIVES.

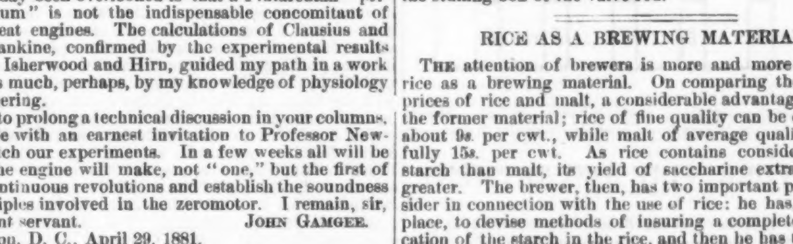
THIS apparatus, which is in current use at present among the English railway companies, is designed for repairing the cylinders of locomotives while they remain in place. When, after prolonged use, a cylinder needs to be bored, its heads are removed from the ends, and the machine shown in Fig. 1 is put in place. The shaft which carries the tool is kept in the axis of the cylinder by means of couplings, which, fixed at its extremities, are bolted to the flanges of the cylinder in the very place that the cylinder-heads occupied. The tool is mounted very much in the same way as in the ordinary boring machines in use. The rotary motion is given by a helicoidal gearing, the endless screw of which is fastened on a fly-wheel when the apparatus is to be operated by hand, but upon a channeled pulley when the power is to be transmitted by a cord. The forward movement of the tool is effected as usual by differential gearings.

The apparatus is made in two sizes. The larger one, whose tool has a travel of 30 inches, can bore cylinders having a diameter of from 12 to 20 inches. The smaller bores cylinders of from 8 to 12 inches in diameter, and its maximum travel is 24 inches.

Fig. 2 represents an apparatus designed for planing and dressing the seats of slide valves. It consists of a cast iron frame on which are mounted two rotary tool carriers actuated by an endless screw and having an automatic longitudinal forward movement. A fly wheel with handle is fixed on the axis of the screw when motion is to be given by hand, but is replaced by a channeled pulley when the power is to be transmitted by rope or cord. The apparatus is held in the interior of the steam chest at one end by an iron cross piece which rests against the sides of the chest, and at the other by a bronze support which is mounted on the screw itself and which has first been put in the place of the stuffing box of the valve rod.



APPARATUS FOR BORING LOCOMOTIVE CYLINDERS IN PLACE.



APPARATUS FOR DRESSING LOCOMOTIVE VALVE-SEATS.

RICE AS A BREWING MATERIAL.

THE attention of brewers is more and more directed to rice as a brewing material. On comparing the respective prices of rice and malt, a considerable advantage rests with the former material; rice of fine quality can be obtained for about 9s. per cwt., while malt of average quality is worth fully 15s. per cwt. As rice contains considerably more starch than malt, its yield of saccharine extract must be greater. The brewer, then, has two important points to consider in connection with the use of rice: he has, in the first place, to devise methods of insuring a complete saccharification of the starch in the rice, and then he has to assimilate the composition of a rice wort to that of one made from malt. In several previous notes we have attempted to explain how rice can be brought into solution. This can be effected by two methods—either by the chemical (acid) process or by the diastasic (malt infusion) process. It will probably be a long time before the prejudice against the chemical process will be altogether overcome; both brewers and the public will object to breweries being converted into chemical manufactories, and therefore the lead-lined tanks and high-pressure chambers necessary for the conversion of starch by this method are not likely to come into general use at present. The saccharification by malt infusion is a process which adapts itself more easily to the brewer's existing plant, and as some malt must always be used in the preparation of good-flavored beer, it is only reasonable to suppose that the diastasic system of conversion will be most generally adopted if rice be used at all. The essential conditions of this system have already been explained in previous notes; the starch must first be brought to a pasty or semi-fluid condition, and must then be acted upon by malt extract at suitable temperatures; to insure complete conversion, the diastasic action will probably have to be sustained for a considerable period at a fixed or gradually increasing temperature. These conditions will certainly necessitate some new forms of plant, but the present appliances of a brewery can be adapted to such requirements with but little difficulty. Assuming the brewer has at his command all the conditions for preparing a saccharine wort from rice, and assuming there is an economical advantage in the use of this material, the brewer has another point to consider, and that is the composition of the resulting wort. Rice is rich in starch, but deficient in albuminoid substances, and therefore a wort made from it will have a similar composition; it is also deficient in the empyreumatic products which give to well-dried malt its peculiar and pleasant flavor. If rice, therefore, is to be used as a partial substitute for malt, it must be in conjunction with another grain capable of yielding those constituents in which it is itself deficient.—*Brewers' Guardian.*

SCARLET FOR FELTS.

THE following two processes give shades which bear soaping. The dyeing is done in a well-tinned pan or a wooden cistern; the goods are entered at 115° F. in water, to which 1½ lb. white argol is added, and boiled strongly for a long time, turning occasionally. Lift, and add the dissolved coloring matter; re-enter, turn, and add gradually, lifting the goods before each addition of 11 lb. tin composition. The beck is then brought to a boil again, which is kept up for half an hour. Lift, cool, and wash well.

If the argol does not loosen the tissue sufficiently, it is recommended to add a small quantity acetate of soda.

The tin composition is prepared as follows:

Muriatic acid	8 lb.
Nitric acid	1 "
Water	1 "

To every 6 lb. of this mixture 1 lb. of granulated tin is added, with the aid of a gentle heat.

Sulphuric acid may be used instead of the tin spirits, but the shades are less pure.

The first method consists in dyeing the goods thus mordanted with the "Ponceau 2 R" of the Aniline Color Company of Berlin. In the second, the goods mordanted in the same way, are dyed with "Ponceau 8 extra," made by the same company.—*Muster Zeitung für Faerberei.*—*Chemical Review.*

CELERITY OF THE NEW YORK FIRE DEPARTMENT.

RECENTLY we published a statement from the Cincinnati *Enquirer* discrediting the report that Engine Company No. 4, of this city, could hitch up in 2¼ seconds, and ridiculing the idea that such quick time could be made by that or any other company. There was some impossible wager tendered by the *Enquirer* contingent upon Engine Company No. 4 visiting Cincinnati and giving an exhibition of quick hitching. It might as well ask to have the New York Post Office or City Hall sent out for exhibition. Frequent statements have been printed as to the celerity of the New York firemen, and all of these have been looked upon with incredulity. We have, therefore, thought it worth while to prepare a diagram of a modern engine house, and to explain why it is that our firemen have attained such proficiency in getting to fires.

In the first place it should be understood that New York is not, in any sense of the word, an exhibition department. None of its apparatus can be taken from the city for exhibition at tournaments or elsewhere, nor can the companies compete with their neighbors or with each other. It is organized purely for hard work, and there is no nonsense about it. Owing to the dangerous construction of our city, its many inflammable buildings crowded thickly together and towering to great heights, the Fire Commissioners made up their minds long ago that immunity from great conflagrations could only be obtained by preventing small fires from becoming large ones. They have, therefore, devoted their attention to moving their apparatus with the greatest attainable celerity. To this end many of the engine houses have been remodeled, the swinging harness has been adopted, the men drilled to secure rapidity in hitching up, and, in fact, everything that was conducive to quick work has been adopted. The necessity for catching fires in their incipiency will be appreciated when it is understood that of the 1,800 fires last year, more than two-thirds of the number occurred in the heart of the city, among our best business blocks and thickly populated districts. A fire raging ten minutes without opposition would result in a wide-spread conflagration imperiling many lives and millions of dollars' worth of property. To secure the prompt response of fire apparatus, they are so located that an engine is due at any given point within two minutes of the sending of the alarm from the street-box; a second one is due at the same point a moment or two later a hook and ladder truck is due simultaneously, and five

engines in all are due at the spot within five minutes. There is much competition between companies, and if the engine that is second due can get there before the one first due, it is a feather in the cap of the former company, and the one that is dilatory must give good and satisfactory reason in his morning report to the chief for his dilatoriness. When such a brief space of time as two minutes only is allowed a company to reach the scene of a fire, they must, of course, be prepared to get out of the house quickly. Every means to facilitate quick hitching has been adopted, and, as a result, there are a number of companies that can hitch up ready to leave the house even quicker than 2½ seconds. This is no "brag" work, nor is it for exhibition, but it is done for every day service. Below we give a diagram of a modernized engine house, showing the positions of the apparatus, the arrangement of stalls, etc.:

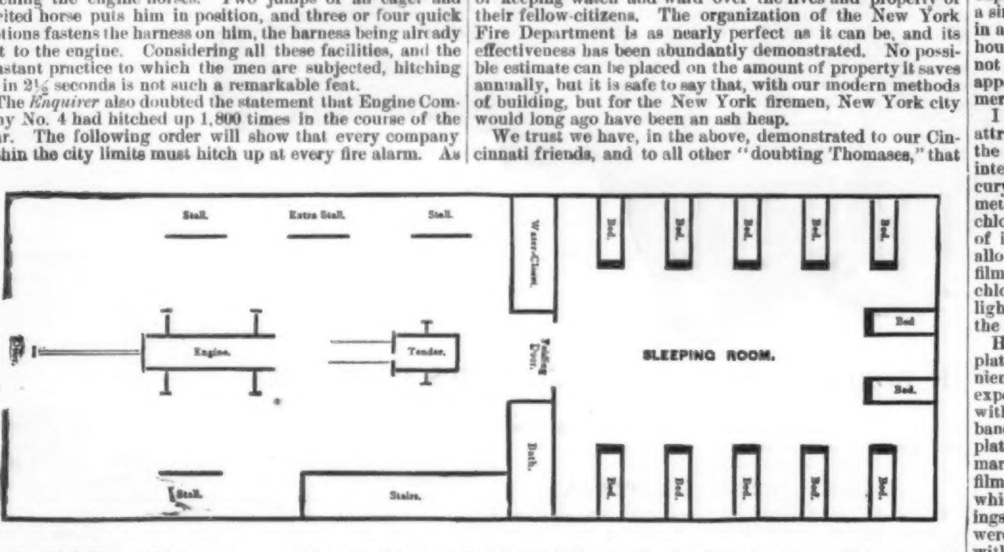
When it is understood that two men are on "floor duty" all the time; that the horses are so trained that they are quite as eager to "go" as the men; that they come to their places at the pole at the first tap of the gong, it will be readily seen that not only is the hitching time named possible, but can be accomplished by any well-drilled company. Over the pole of the engine is suspended the harness; the horses come to the pole, the harness is dropped upon their backs, a single motion clasps the collar, the cross reins are snapped in the bits, and the horses are both harnessed and hitched. Every man knows his place, and just what he has to do, and does it promptly. At the first stroke of "the joker"—the small telegraph instrument—each man and horse jumps to his place, the driver mounts to his seat, and as the horses are hitched, he cries out "ready." If the alarm comes from a station to which the company responds, the captain gives the word "go," and the apparatus is driven out on the dead run. At night, the same routine is observed, the two floor men attending to the hitching, while the others are tumbling out of bed, and getting into position on the engine or tender. The tender is the hose carriage, and the hitching of the single horse in the shafts is conducted in the same manner as in hitching the engine horses. Two jumps of an eager and excited horse puts him in position, and three or four quick motions fastens the harness on him, the harness being already fast to the engine. Considering all these facilities, and the constant practice to which the men are subjected, hitching up in 2½ seconds is not such a remarkable feat.

The *Enquirer* also doubted the statement that Engine Company No. 4 had hitched up 1,800 times in the course of the year. The following order will show that every company within the city limits must hitch up at every fire alarm. As air is provided, and abundant ventilation, while neatness and order are peremptorily enforced.

During the year 1880, the fire alarms in New York averaged one in every five hours, and during some months the average was one in three hours. These alarms are about equally divided between day and night. At every alarm, as we have shown, every apparatus is hitched up ready to respond if necessary. The men are in position prepared for a run, and they remain so until the last stroke of the big gong assures them that it is not a station to which they respond, when they unhitch and go to rest. If it is one of their stations, however, they do not wait for the big gong to strike, but run as soon as the small instrument has struck one round. It thus frequently happens that a company is two or three blocks away before a single round has been struck on the big gong. Thus at every alarm every fireman, every horse and every apparatus in the city—being 735 men and 240 horses—are ready to move immediately to the point threatened. It may seem unnecessary that when a fire occurs at the Battery, for instance, the apparatus in Harlem, ten miles distant, should be required to be in readiness, but if the fire at the Battery should prove a serious one, a third alarm would bring twenty-five or more engines to the scene, from the thickly populated districts. To afford protection to these districts thus left uncovered, the apparatus from the suburbs would be moved down town to be ready for duty in case a second or third fire should break out. It is only by this unceasing vigilance and great celerity that New York is spared the disaster of a great conflagration.

From what we have said, it will be seen that the position of a fireman is not a sinecure. They are on duty all the time, twenty-four hours in the day. Undisturbed sleep is unknown to them, regular meals are impossible, for when at a fire they cannot leave for meals or anything else. In addition to fire duty, they must groom their horses, take care of their apparatus, of their quarters, clean and dry hose, make inspections in their districts of buildings, hydrants fire alarm boxes, and perform various other duties, in the nature of keeping watch and ward over the lives and property of their fellow-citizens. The organization of the New York Fire Department is as nearly perfect as it can be, and its effectiveness has been abundantly demonstrated. No possible estimate can be placed on the amount of property it saves annually, but it is safe to say that, with our modern methods of building, but for the New York firemen, New York city would long ago have been an ash heap.

We trust we have, in the above, demonstrated to our Cincinnati friends, and to all other "doubting Thomases," that



Depth of Building, inside measurement, 85 feet. Width of Building, 22½ feet. Depth of Engine-Room, 50 feet. Head of Tongue from Door, 22 inches. Horses' Heads from Head of Tongue, 12 feet. Engine Wheels from Stalls, 5 feet.

MODERN ENGINE HOUSE OF THE NEW YORK FIRE DEPARTMENT.

there were 1,800 alarms last year, every company was hitched up that number of times:

NEW YORK, November 14, 1879.

[General Orders No. 8.]

When any signal on either the gong or talking circuit, or any indication of fire (in whatever manner the same may be communicated), is received at any company quarters, every officer and member will immediately report for fire duty on the apparatus floor, the horses will be hitched up and the company prepared to leave quarters upon the command "Go!" to be given by the commanding officer at the instant he is assured that the company performs duty at the location indicated. When it is ascertained that the company does not perform duty at the station or point indicated, the commanding officer may give the command to unhitch immediately, except in case of a telegraphic alarm, in which case he will hold the company at attention until the last round of the signal number has been sounded upon the gong.

By order of the Board,

VINCENT C. KING, President.

As a matter of fact, however, every company hitched up three or four times 1,800 times. Twice a day—morning and night roll call—the whole operation is gone through with, in addition to which those houses that have been remodeled, have many visitors, and their hitches to satisfy the incredulous will average four or five a day. So the men are in constant practice. But all the engine houses have as yet not been rebuilt, as shown in the above diagram. In some the horses are in stalls in the rear of the building, and the men's quarters are on the second floor. Of course these companies cannot hitch so quickly, but few of them require more than five or six seconds. The time depends mainly upon the distance the horses have to run. But the commissioners are remodeling the houses as fast as they can get the money to do it, and it will not be long before they are all modernized.

It may be thought that the arrangement for men and horses sleeping on the same floor would make it unhealthy for the men. But such is not the case. The cellar is dug out the whole depth of the lot, and thoroughly ventilated. There is a ventilator for the engine room, and another for the sleeping room, while both rooms are kept as clean as a parlor floor. The beds of the men are two and a half feet above the floor, which is above the carbonic gases that may be generated in the winter, and below them in the summer, as these gases ascend in hot weather, and descend in cold. At the height of the beds from the floor, the men experience no ill effects from the atmosphere, and medical men say that the arrangement is not deleterious to health. Great care has been taken in this respect, for the health of the men is one of the first things to be considered. Plenty of fresh

it is not only feasible for our firemen to hitch up in 2½ seconds, but that there is no exaggeration in the statement as to the number of times they hitch up ready for a run in the course of a year. Still, if those Cincinnatians who talked so freely about investing money on the result, are still skeptical, let them appoint a committee to come to New York and see the hitching done, and we will not only guarantee better time than 2½ seconds, but will find takers for whatever shekels they desire to invest in that direction.—*N. Y. Fireman's Journal.*

We may add that during the recent visit of the Duke of Sutherland in New York, the Fire Commissioners, on the evening of April 27, entertained that nobleman and his companions as follows:

The Duke of Sutherland, Mr. Russell, the correspondent of the London *Times*, and about fifty residents of the city, among whom were Fire Commissioners King and Van Cott, visited the station of Engine Company No. 14, in Eighteenth street, and inspected the engine, horses, etc. The firemen hitched up several times, as if to answer an alarm of fire, and their time was from two and a half to three seconds. The Duke wrote his name in the station "blotter," with the remark, "Very smart," and Mr. Russell wrote, "Very much pleased, indeed." After leaving the engine house the party went to Twelfth street and Fifth avenue, and there Battalion Chief Giquel, by direction of the Fire Commissioners, pulled the alarm box. In two minutes and four seconds Engine No. 14 was on the spot. Thirty-six seconds later Engine No. 18, from West Tenth street, was on hand. Engine No. 33, from Mercer street, near Third street, was only a minute later in arriving. Two hook and ladder companies and the insurance patrol came up about the same time. The firemen were sent back to their stations, and then Chief Giquel gave a special call for the self-propelling engine, stationed in Morton street, near Hudson street. In six minutes the propeller was seen passing the Brevoort House. After reaching Twelfth street, it was stopped and the Duke was allowed to see it pump a large stream of water to a considerable distance in the air. He expressed himself as greatly delighted with the work of the firemen.

MAGIC MIRRORS.—The magic mirrors, which have been a good deal discussed of late, are all of metal. M. Laurent has succeeded in making them of glass, which is sufficiently elastic for the purpose. At first he used pressed glass, polishing the surface opposite to the projections; then he tried the thin glass of commerce, engraving a hollow design. The two methods may be combined. When at rest the mirror is plane, and gives good images. By a blowing or sucking action the characteristic features are brought out. Both sides of the mirror are silvered.

STRAY NOTES ON GELATINE PLATES.

By E. BRIGHTMAN.*

AFTER all that has been written on the subject of gelatine plates during the past two years it may be thought the matter has been well-nigh exhausted, and any further allusion to the subject suggests a repetition of what has been said again and again.

I, however, do not intend to revert to the subject of the preparation of the gelatine plate, which has occupied so large a share of attention, but merely to give the results of a few experiments, and an account of various odd matters, hastily jotted down in odd moments during the past season. I feel that an apology is due to our association for bringing forward such a jumble of disconnected odds and ends, but knowing that a profitable discussion is often the result of a few simple remarks, I venture for a few minutes to occupy your time, with the hope that our after discussion may be more interesting and profitable than my remarks.

Much doubt has been expressed as to the permanency of gelatine negatives intensified with mercury, all negatives in which mercury has been used being condemned as liable to fade. For my part, I am inclined to think that the charge of fading is unfounded, and consider, on the contrary, that instead of fading there is far more probability of such negatives becoming more and more dense from the action of light on the film during printing.

There are so many methods of intensifying with mercury, that even though some may result in fading, or an increase of density, others may be, and are, as I can show by actual results, not liable to fade or change in any way.

The negative I now pass round was taken in September last, and intensified by immersion, first in bichloride of mercury, and afterward in a dilute solution of ammonia: one half has been covered with a thick piece of cloth, and the other half fully exposed to daylight, yet no difference whatever is observable between the shielded half and the half exposed to the light. The other negative was intensified in a similar manner, the plate cut in two, one half being kept in a dark drawer, and the other half exposed on a greenhouse shelf to full daylight; on putting the parts together not the slightest difference is observable. From this it appears that some methods, at least, of intensifying with mercury do not cause fading or increase of density.

I am inclined to think that the want of permanence attributed to negatives intensified with mercury arises from the fact that wet plates undoubtedly did change when intensified by the old method of first bleaching with mercury, and afterward applying pyrogallic and silver; by this method the silver in the film would be converted into chloride of silver, and in the event of only a slight amount of intensification being required, the pyrogallic would be allowed to act for a few seconds only on the surface of the film, leaving underneath a portion of unreduced silver chloride, which on printing would, from the action of the light, become more and more darkened, and at last render the negative totally useless.

Having during the past summer used a number of gelatine plates at the seaside, and not having the necessary conveniences for developing away from home, the plates, after exposure, were repacked in parcels, and kept from contact with one another by slips of white paper, and secured by bands of the same paper placed across the center of the plates, and held with twine. On developing the plates, markings corresponding with the paper were found on the film. At first I concluded the paper contained some impurity which, coming in contact with the film, produced the markings in question; on further examination, however, marks were found where the paper had not been in actual contact with the film. At first the matter considerably puzzled me, but after considering the matter and carefully examining the plates, I was led to the conclusion that the white paper, having been for some time exposed to a strong light, was capable of impressing its image on the sensitive film. It is said that if a piece of white paper is exposed to a strong sunlight and at once taken into absolute darkness, it will be found to be faintly luminous. If so, is it not possible, and even probable, that paper so exposed may be sufficiently luminous to impress itself on the highly sensitive gelatine film?

I presume most of our members are acquainted with the phosphoroscope; but, as it may be new to some, I venture to show what I consider a most interesting experiment, as it illustrates in a most striking manner the property possessed by certain substances of absorbing and afterwards emitting luminous rays.

The phosphoroscope consists of a series of small glass tubes, containing the sulphides of calcium, barium, magnesium, and other sulphides. These tubes, as you will observe, are perfectly invisible in the daylight; but I will now expose them to the light of a piece of burning magnesium, and you will observe they will emit a brilliant glow of light of various colors—calcium giving one color, barium another, magnesium another, and so on.

Now it appears to me as highly probable that other substances besides these sulphides may possess the property of absorbing and afterward emitting luminous rays; and again, is it not possible that some substances may possess the power of absorbing the invisible rays of the spectrum, and although these rays, if afterward emitted, would not be perceptible to the eye, yet they may have the power of acting on the sensitive film?

Having a great dislike to the use of hyposulphite of soda in the dark room, I have always considered the necessity of fixing before exposure to the light. One of the drawbacks of the gelatine process in all directions for the development of gelatine plates, the necessity of fixing before allowing the light to have access to the plate, is strongly insisted upon. I, however, find in practice that if the developer is washed from the plate, light has no further action on the film. As a proof of this I produce several negatives, all of which were fixed in full daylight; yet the shadows are, in every case, perfectly clear.

The question as to whether a greater latitude of exposure is admissible with dry plates than with wet is one which has been somewhat disputed. I fully expressed my views on the subject at a previous meeting, but since then have put the matter to a practical test.

In order to do so, I availed myself of the simple yet ingenious contrivance, which was, I believe, the idea of Captain Abney, consisting of a small plate of glass divided into a number of small squares, numbered (say) from one to sixteen, number one square being covered with one thickness of tissue paper; number two with two thicknesses; number three with three thicknesses; and so on, increasing one thickness with each square.

* Read before the Bristol and West of England Amateur Photographic Association.

This little instrument was constructed by Captain Abney for the purpose of testing the relative sensitiveness of various plates, that plate which would give the greater number of tints in a given time and with a given light being the most sensitive.

Thinking the matter over, the idea occurred to me that the same contrivance might be used for deciding the question of latitude of exposure, for it will be evident that the plate which will give an image under the greatest number of thicknesses of paper, without over exposure on the square with a single thickness, is the plate which admits of the greatest latitude of exposure.

Putting the matter thus fairly to the test, I find wet plates decidedly have the advantage. The dry plates tested against the wet were of two different makes, and, contrary to my anticipations, the most sensitive make of dry plates gave the greater latitude of the two; and from the few tests I made, it appeared that the more dense the film, the greater the latitude of exposure admissible.—*Photographic News.*

HANDKERCHIEF EXTRACTS.

By ROBERT H. COWDREY, Ph.G.

THE writer proposes to give to the patrons of the *Pharmacist* his private formulas for the manufacture of perfumes, which he has used for the last five years, and in that time has not found it necessary to purchase an ounce of bottled or bulk extracts from outside sources, which of itself is reason enough to induce each pharmacist to try these formulas; and as there is nothing which is difficult or needs great experience in their manufacture, the writer hopes they will be given a careful trial. The writer does not claim the entire originality of the process or combinations, but gives his with those he has selected from other writers, among them Piesse & Lubin. As their cost, as compared with manufacturers' prices, is of vital importance, the cost of articles entering into their composition will be stated, and on this basis the cost of each odor per pint.

Handkerchief extracts, when of good quality and reasonable cost, are a valuable addition to a druggist's stock, and by making them in his own laboratory he saves the extra cost of handling them, and is in command of the quality of the goods he desires to sell.

In this article "essence" will denote the first washings of the pomade; the usual method of obtaining them being to add to each pound of "Chiris" No. 24 pomade 16 ounces of alcohol, or to "Chiris" No. 30 pomade 20 ounces alcohol. Place both in a wide mouthed jar or can, and with the hand thoroughly mix; set aside, keeping the container tightly covered; in four or five days again thoroughly mix, and for two weeks or longer this should be done each four or five days. Then pour off the clear liquid and press from the pomade as much as possible of the remainder. Then add to the pomade the same amount of alcohol and macerate in the same way as before. Strain out, and of this second washings use enough to make the first washings measure either 16 or 20 ounces, according as No. 24 or 30 pomade was used. This, which is known as first washings, must now be thoroughly chilled in order to congeal the lard, which can then be separated by filtering, which being done, the liquid is finished and ready for use.

Extract will denote the finished liquid odor.

Spirit will denote an alcoholic solution of an essential oil.

Tincture will denote a solution of the drug it represents containing the portion which is soluble in the liquid.

Alcohol will denote only deodorized alcohol.

The following enter into the formulas to be given below:

Alcohol	cost	$2.20 per gall.
Ambergris	"	3.00 oz.
Civet	"	3.00 oz.
Oil or Otto Bergamot	"	3.00 lb.
Oil Lavender (Mitchanes)	"	2.25 oz.
Oil Orange Flowers	"	40 drachm.
Oil Rose Geranium	"	1.25 oz.
Oil Ylang Ylang	"	9.00 oz.
Pomades, No. 24, Violet	"	4.50
Pomades, No. 24, all other odors,	"	2.40
Benzoin	"	50 lb.
Grain Musk	"	3.25 oz.
Orris Root	"	35 lb.
Oil Cloves	"	2.25 lb.
Oil Almonds (Bitter)	"	50 oz.
Oil Lemon	"	2.25 drachm.
Oil Patchouli	"	1.65 oz.
Oil Rose	"	6.50 oz.
Oil Sandalwood	"	50 oz.
Oil Vetivert	"	10.00 oz.

ORANGE FLOWER SPIRIT.

Mix Orange Flower Otto	40 minims.
Alcohol	8 ounces.

CLOVE SPIRIT.

Mix Clove Otto	20 minims.
Alcohol	4 ounces.

VANILLA TINCTURE.

℞. Vanilla Beans	6 troy drachms.
Alcohol	1 pint.

Beat the vanilla to coarse power, macerate with gentle heat for four hours and filter; while macerating keep a wet towel over mouth of the bottle, using a water bath.

ROSE SPIRIT.

Mix Rose Otto	50 minims.
Rose Geranium Otto	40 "
Alcohol	8 ounces.

VETIVERT SPIRIT.

Mix Vetivert Otto	30 minims.
Alcohol	4 ounces.

YLANG YLANG SPIRIT.

Mix Ylang Ylang Otto	80 minims.
Alcohol	8 ounces.

AMBERGRIS TINCTURE.

℞. Ambergris (Gray)	30 grains.
Orris Rt. Powd	1 drachm.
Alcohol	8 ounces.

Beat the ambergris with the orris root to a powder, then add the alcohol and macerate for 30 days, with occasional agitation, and filter.

BENZOIN TINCTURE.

Mix Benzoin	2 ounces.
Alcohol	1 pint.

CIVET TINCTURE.

℞. Civet	30 grains.
Orris Rt. Powd	1 drachm.
Alcohol	8 ounces.

Triturate the civet with the orris root until thoroughly mixed, then add the alcohol and macerate for 30 days, with occasional agitation, and filter.

MUSK TINCTURE.

℞. Tonquin Grain Musk	1 drachm.
Hot Water	4 drachms.
Alcohol	1 pint.

Digest the musk in the hot water for three or four hours, then add the alcohol and macerate for 30 days, with occasional agitation, and filter.

ORRIS TINCTURE.

℞. Orris Rt. Powd	2 ounces.
Alcohol	4 "

Macerate the orris root for seven days and filter, then percolate the orris root with alcohol sufficient to make the measure up to 4 fluid ounces.

ALMOND SPIRIT.

Mix Almond Otto (Bitter)	40 minims.
Alcohol	8 ounces.

EXTRACTS.

WHITE ROSE EXTRACT.

Mix Rose Spirit	4 ounces.
Violet Essence	2 "
Jasmin Essence	2 "
Patchouli Extract	¼ ounce.
Costs	$1.76 per pint.

YLANG YLANG EXTRACT.

Mix Ylang Ylang Spirit	8 ounces.
Jasmin Essence	8 "
Costs	$3.44 per pint.

JOCKEY CLUB EXTRACT.

Mix Tuberose Essence	2 ounces.
Rose Spirit	2 "
Rose Essence	2 "
Ambergris Tincture	1¼ "
Civet Tincture	2 drachms.
Musk Tincture	2 "
Bergamot Otto	30 minims.
Clove Otto	10 "
Costs	$1.84 per pint.

JASMIN EXTRACT.

Mix Jasmin Essence	4 ounces.
Vanilla Tincture	¼ ounce
Ambergris Tincture	2 drachms.
Costs	$2.24 per pint.

FRANGIPANNI EXTRACT.

Mix Tuberose Essence	1 ounce.
Vetivert Spirit	¼ ounce.
Sandal Otto	15 minims.
Rose Otto	15 "
Orange Flower Otto	15 "
Alcohol	¼ ounce.
Musk Tincture	2 ounces.
Orris Tincture	1 ounce.
Orange Flower Essence	1 "
Costs	$3.00 per pint.

HELIOTROPE EXTRACT.

Mix Orange Flower Essence	1 ounce.
Rose Spirit	1 "
Vetivert Spirit	2 ounces.
Vanilla Tincture	1 ounce.
Orris Tincture	2 ounces.
Tonka Tincture	1 ounce.
Orange Flower Spirit	1 "
Ambergris Tincture	4 drachms.
Sandalwood Otto	10 minims.
Clove Otto	4 "
Costs	$1.50 per pint.

TUBEROSE EXTRACT.

Mix Tuberose Essence	4 ounces.
Orris Tincture, Ambergris Tincture,	each ¼ ounce.
Costs	$2.24 per pint.

WEST END.

Mix Rose Spirit	3 ounces
Benzoin Tincture, Musk Tincture,	each 1 ounce.
Verbena Extract, Civet Tincture,	each ¼ "
Sandalwood Otto	10 minims.
Costs	$1.65 per pint.

PRINCESS BOUQUET.

Mix Bergamot Otto, Clove Otto.	each ¼ drachm.
Lavender Otto	1 "
Musk Tincture, Vanilla Tincture, Ambergris Tincture,	each 2 drachms.
Rose Spirit	1 oz. and 2 drs.
Alcohol	8 ounces.
Costs	$1.03 per pint.

BRIDAL BOUQUET.

Mix Vanilla Tincture	2 drachms.
Musk Tincture, Benzoin Tincture, Orris Tincture,	each 1 drachm.
Cassie Essence	4 ounces.
Tuberose Essence, Jasmin Essence,	each 2 "
Bergamot Otto	16 minims.
Orange Flower Otto	6 "
Costs	$2.35 per pint.

VIOLET EXTRACT.

Mix Violet Essence	4 ounces.
Cassie Essence	1 ounce.
Rose Essence	3 drachms.
Orris Tincture	1 ounce.
Ambergris Tincture	2 drachms.
Civet Tincture	2 "
Almond Spirit	20 minims.
Costs	$2.90 per pint.

VERBENA EXTRACT.

Mix Verbena Otto True	1 drachm.
Lemon Otto	1 "
Alcohol	8 ounces.
Costs	$1.00 per pint.

ESS. BOUQUET.

Mix Rose Spirit	2 ounces.
Ambergris Tincture	2 drachms.
Orris Tincture	1 ounce.
Bergamot Otto	1 drachm.
Lemon Otto	15 minims.
Costs	$1.32 per pint.

PATCHOULI EXTRACT.

Mix Patchouli Otto	2 drachms.
Rose Otto	20 minims.
Alcohol	15 ounces.
Costs	$0 96 per pint.

HONEYSUCKLE EXTRACT.

Mix Patchouli Extract	3 drachms.
Benzoin Tincture, Rose Essence, Clove Spirit,	each ¼ ounce.
Civet Tincture, Orange Flower Spirit,	each 1 "
Jasmin Essence	4 ounces.
Vanilla Tincture	1 ounce.
Costs	$1.50 per pint.

CLOVE PINK EXTRACT.

Mix Clove Spirit	2 drachms.
Vanilla Tincture	¼ ounce.
Violet Essence	¼ "
Orange Flower Spirit	1 "
Rose Spirit	2 ounces.
Costs	$1.35 per pint.

SANDALWOOD EXTRACT.

Mix Sandalwood Otto	3 drachms.
Rose Otto	20 minims.
Alcohol	8 ounces.
Costs	$1.25 per pint.

SPRING FLOWERS EXTRACT.

Mix Rose Essence	2 ounces.
Tuberose Essence	2 "
Rose Spirit	2 "
Musk Tincture	¼ ounce.
Ambergris Tincture	1¼ "
Clove Otto	10 minims.
Bergamot Otto	¼ drachm.
Costs	$2.60 per pint.

MUSK EXTRACT.

Mix Musk Tincture	2 ounces.
Civet Tincture	2 "
Rose Otto	10 minims.
Alcohol	1 ounce.
Costs	$2.26 per pint.

This extract of musk is a more pleasant and of a more natural musk odor than any I have been able to make from the grain musk alone.

NEW MOWN HAY EXTRACT.

Mix Moss Rose Extract	1 ounce.
Benzion Tincture	1 "
Tonka Tincture	4 ounces.
Musk Tincture	1 ounce.
Rose Geranium Otto	40 minims.
Bergamot Otto	40 "
Alcohol	1 ounce.
Costs	$1.50 per pint.

FLORAL BOUQUET.

Mix Musk Tincture	2 ounces.
Orris "	6 drachms.
Tonka "	6 "
Vanilla "	6 "
Ambergris Tincture	1 ounce.
Rose Spirit	4 ounces.
Costs	$1.05 per pint.

MOSS ROSE EXTRACT.

Mix Rose Spirit	3 ounces.
Orange Flower Essence	1 ounce.
Ambergris Tincture	¼ "
Musk Tincture	2 drachms.
Costs	$1.75 per pint.

RONDOLETIA EXTRACT.

Mix Lavender Otto (English)	1 drachm.
Clove Otto	15 minims.
Bergamot Otto	30 "
Musk Tincture	2 drachms.
Vanilla Tincture	2 "
Ambergris Tincture	2 "
Rose Spirit	1¼ ounces.
Alcohol	8 "
Costs	$1.10 per pint.

I have endeavored to place the extracts in their order as to value, taking into consideration their quality, permanence, and notoriety.

Thus, White Rose being more widely known is more often called for than Spring Flowers.

The Princess and Bridal Bouquets, though good extracts, are not sold as often as Violet and Ylang Ylang.

The first ten make a good assortment, but the others make valuable additions.

It is very essential to use only the best oils.

The second washings can be used to good advantage as a base for colognes, or in a cheaper grade of odors, but this last is not recommended. All perfumes should be kept from the direct rays of sunlight, and all essential oils in as cool a place as possible, and in the dark.—*The Pharmacist.*

ON THE REMOVAL OF AQUEOUS VAPOR FROM THE ATMOSPHERE.*

By J. J. COLEMAN, F.I.C., F.C.S.

THE absolute weight of moisture contained in any given volume of air, and at any particular temperature, is usually calculated from a table of vapor tensions by a formula well known to meteorologists, so that the accuracy of the results depends upon the care with which the table of vapor tensions has been compiled from direct experiment. Fortunately for this, as well as other branches of physics, we have the exact experiments of Regnault, which, in the case in point, were carried down to about 20 deg. below zero of Fahr. scale; but as at that temperature the tension of water vapor is only 0·017 in. of mercury, it is quite obvious that errors of experiment would be apt to increase to a serious extent in carrying observations to lower temperatures by the method adopted by this experimentalist. One of the earliest papers that the late Professor Rankine wrote was one on the elasticity of vapors—*Edinburgh New Philosophical Journal*, July, 1849—in which he says: "I have obtained among other results an equation giving a very close approximation to the maximum elasticity of vapor in contact with water," and from three constants, viz., the vapor tension at 220 deg. Cent., at 100 deg. Cent., and at 26 deg. Cent., he calculated theoretically the vapor tensions for every 10 deg., from 230 deg. to 20 deg. below zero, which correspond almost exactly with Regnault's experiments. In reference to this formula, Professor Rankine observed that it may be employed without material error, for a considerable range beyond what he proved it, but that it can only be regarded as an approximation to the exact physical law of the elasticity of vapors, for the determination of which many constants are still wanting, which can only be supplied by experiment. The principal point involved in such an inquiry is the question as to whether aqueous vapor ceases to have elasticity at any point short of absolute zero. Passing, however, from such remote considerations, and directing attention to the absolute weight in grains per cubic foot of vapor at various temperatures, I have been led to notice the ratio in which vapor is liquefied by regular diminutions of temperature from 100 deg. Fahr. above zero, down to zero itself.

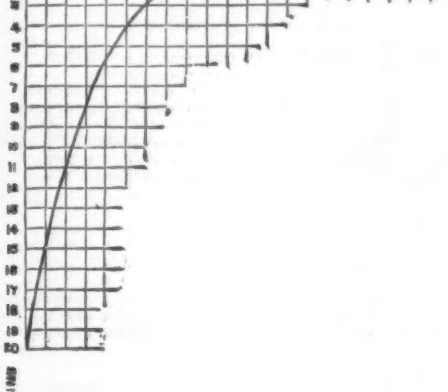
pendent influence of the freezing point of water upon this curve, although there is a little irregularity. There is no sudden deposition of moisture when the freezing point is attained, ice, in fact, imparting humidity to air just as water had previously done before the freezing point was attained.

With a view to consider for a moment the joint effect of cold and pressure upon aqueous vapor, I have now to remind you of a well-known law of physics, viz., that when saturated vapor is subjected to pressure it will liquefy in the direct ratio of the pressure, temperature being constant; and also that atmospheric air saturated with aqueous vapor behaves in this respect just the same as if the air were not present. This principle was illustrated by Dalton, who introduced volatile acids into the Torricellian vacuum of a barometer tube, and showed that the liquids evaporated or recondensed in proportion to the elevation or lowering of the tube in a mercurial trough. Assume, then, that air at 60 deg. Fahr., and saturated with moisture, is compressed to 20 atmospheres, and in a surface condenser, consisting of a suitable system of tubes surrounded by an ample supply of water at the initial temperature of the air, then nineteen-twentieths of the weight of that aqueous vapor should be deposited as dew in the inside of the pipes. If the volume of the air at starting were 1 cubic foot at 60 deg., then it would contain 5·8 grains of water, and when compressed to 20 atmospheres without change of temperature, 5·5 grains would be deposited, and being expanded again to its original volume and pressure, out of contact with the deposited water, it would be found to contain only three grains of water.

Going a step further, let us suppose that the same cubic foot of vapor-saturated air at 60 deg. is compressed into one twentieth its bulk in another way, viz., in direct contact with water, say, by forcing it into a strong reservoir partially filled with water. Imagine the compressed air and water to be shaken together, and then allowed to stand until perfectly quiescent, the temperature being kept at 60 deg.; now let the water be carefully drained away or detached from the compressed air, and the air be expanded to its former bulk, and it will be found to be drier than it was at the start, as it will have lost nineteen-twentieths of its vapor just as in the

	Glaisher's tables above zero. Fah.											Calculated below zero. Fah.											
Temperatures	100	90	80	70	60	50	40	30	20	10	0	10	20	30	40	50	60	70	80	90	100	110	120
Weight of cubic foot of saturated vapour in grains.	19·84	14·85	10·98	8·01	5·77	4·10	2·86	1·97	1·30	0·84	0·55	·36	·23	·14	·08	·05	·02	·017	·009	·005	·003	·0015	·001
Percentage of weight deposited for fall of 10 deg. in temperature	25	26	27	28	29	30	31	34	33	34·5	[illegible]	36	37	38	39	40	41	42	43	44	45	46	47

On the tabular statements accompanying this paper I have given two horizontal lines of figures, the upper line up to half its length containing the actual weight in grains of a cubic foot of saturated vapor, as given in Glaisher's Hygrometric Tables, and for temperatures which decrease at the uniform rate of 10 deg. down to zero. Directly underneath these figures I show the rates in which the weight decreases for every drop of 10 deg.; thus, saturated vapor in dropping from 100 to 90 deg. deposits 25 per cent. of its weight; from 90 deg. to 80 deg., 26 per cent. of its weight; from 80 deg. to 70 deg., 27 per cent. of its weight; and so on, the ratio increasing almost uniformly at the rate of 1 per cent. every fall of 10 deg., so that by the time the temperature gets to 10 deg. above zero it parts with 35 per cent. of its weight in falling 10 deg. lower to zero.



It seems reasonable, therefore, to suppose that some similar ratio of decrease will maintain for temperatures far below zero, and in accordance with this view I have ventured to extend the line of figures to a temperature of 120 deg. below zero, from which I have calculated the figures on the remaining half of the line above alluded to, thus showing the probable weight of a cubic foot of vapor for every 10 deg. to 120 deg. below zero. The result can, of course, only be considered as an approximation, for in reality the ratio of liquefaction must be accelerated to insure complete liquefaction at a point above absolute zero; but at any rate it is very clear that at a temperature of 120 deg. below zero a cubic foot of saturated aqueous vapor does not weigh more than the thousandth part of a grain or one five-hundred-and-thirty-six-thousandth part of the weight of the same volume of dry air at 60 deg., or about one eight hundred-thousandth of the weight of a cubic foot of dry air at 120 deg. below zero.

I have also thought it might be interesting to put the result in the form of a graphic curve, the vertical figures representing the weight of a cubic foot of vapor, and the horizontal figures representing the temperature, commencing at 100 deg. above zero, and ending at 100 deg. below zero. One of the most curious facts that strikes the eye is the inde-

former case. Thus we are brought face to face with a curious paradox—that it is possible to dry air by wetting it.

Both of the methods of drying air I have thus described are limited in practice by the difficulty on the one hand of getting temperatures under 100 deg. below zero, and on the other hand of compressing air in a continuous current to higher pressure than the twenty atmospheres; but it is manifest that if the two operations be combined, air might be dried so as not to contain more than a ten-millionth part of its weight of vapor. It is an interesting question—how these figures compare with the result of desiccating air by chemical methods. According to H. D. Debbit, an abstract of whose paper on the subject appeared in the Journal of the Chemical Society, October, 1876, anhydrous phosphoric acid is the most powerful desiccating agent, and he states that this substance will remove the two-millionth part of the weight of air in the form of moisture, even after it has been carefully dried by sulphuric acid at temperatures not exceeding 25 deg. C. When the anhydrous phosphoric acid, he says, was made to act upon air which had been previously dried over sulphuric acid at 50 deg. C., no less than the one-millionth part of its weight proved to be aqueous vapor.

Calcium chloride seemed to be a worse desiccator than sulphuric acid, or, at any rate, its power of desiccation seems to be within very small ranges of temperatures, as the author observes, that if air be dried by passing over this salt at a given temperature, and be brought in contact with a fresh quantity of the salt at a lower temperature, a further absorption of water takes place, but that if the second portion of chloride calcium be maintained at a higher temperature than the first, the air becomes moister. In reference to this subject it may be interesting to refer to the paper of Professor Tyndall, recently read to the Royal Society, "Upon the Action of an Intermittent Beam of Radiant Heat upon Gaseous Matters," in which he describes experiments made by passing radiant heat through flasks containing varying quantities of aqueous and other vapors. Tyndall shows that the more vapor present in air, the more easily are the heat pulsations converted into audible noise, on the principle of Graham Bell's recent discoveries. Dry air, in fact, gave no sounds whatever which could be detected, while minute quantities of many vapors added to the air occasioned sounds which could be compared in intensity with those of an organ pipe.

Among other experiments he mentions that Professor Dewar supplied him with four flasks, the first containing air dried by chloride of calcium, the second one dried by strong sulphuric acid, the third by Nordhausen acid, and the fourth by phosphoric anhydride; and curious to say, the flask containing the phosphoric anhydride emitted the strongest sound, which is the exact reverse of what should be the effect if phosphoric acid were the best desiccant. Even with sulphuric acid the extreme difficulty of drying it was very evident, for Tyndall remarks that air kept over the surface of this acid for twelve hours emitted sounds, which, however, entirely disappeared when the time of contact was increased to eighteen hours. This beautiful method of investigation will doubtless be followed up, and it is to be hoped will clear up many points connected with the relative efficiency of desiccants. In regard to the strictly mechanical method of drying air described in the first part of my paper, it has been to some extent practically carried out in the construction of Bell-Coleman cold air machines used for the oceanic conveyance of meat and other provisions.

In these powerful machines, and of the size most usually employed in the Transatlantic traffic, about 36,000 cubic feet per hour of atmospheric air is taken into the compressors, and supposing this air is two-thirds saturated, and of a temperature of 80 deg. Fahr., it contains 37½ lb. water vapor, some of which must be removed before the air is finally discharged from the machine below zero, or the discharged air would become loaded with clouds of snow, which would be a great practical inconvenience. But in point of fact about half this aqueous vapor is at once deposited and mingles with the water, which is freely injected into the compressors to keep down the heat produced by the compression, and escapes therefrom by a pipe controlled by a ball cock before the compressed air is allowed to expand. It is made to traverse a number of small pipes, the external surface of which are cooled by the waste cold air—say of 30 deg.—coming from the provision room, being refrigerated, so that by this means a very considerable cooling of the compressed air is effected, causing a further liquefaction of vapors, by which, in fact, its quantity is practically halved; thus by the time the air gets to the expansion cylinder, where expansion takes place in the act of doing work, the air, although it has been freely washed with fresh water, contains only about one-fourth of the aqueous vapor which it contains at the start of the cycle, and can be expanded without producing any inconvenient amount of snow. The temperature at the moment of expansion is generally from 30 deg. to 50 deg. below zero, or 100 deg. below zero, when the machine is worked at about four atmospheres of condensation.

This method of producing cold dry air has not only been employed in cold air machines working across the Atlantic, but has also been recently found to work well with machinery traversing the Red Sea and Indian Oceans.

* Read lately before the Chemical Section of the Glasgow Philosophical Society.

ACTION OF BACTERIA ON GASES.

AT a recent meeting of the Chemical Society, London, Mr. F. Hatton read a paper "On the Action of Bacteria on Various Gases."* The experiments were made to ascertain the nature of the action exerted by various germs on the life and increase of bacteria, and to observe what influence the bacteria had on the percentage composition of the gases. The bacteria were obtained by shaking fresh meat with distilled water. The aqueous extract was filtered and exposed to the air for twenty-four to thirty-six hours; it was always found to be full of bacteria. A small flask was half filled with mercury, filled up with the bacteria solution, and inverted in a mercury trough. The gas under examination was then passed up, a small glass vessel was introduced under the mouth of the flask, and the whole removed from the trough. The liquid was examined daily as to the condition of the bacteria, the sample being removed by a piece of bent glass tubing having an India-rubber joint. After about a week the gas was pumped out by means of a Sprengel and analyzed. Atmospheric air was first tried. The bacteria lived well during the fifteen days of the experiment (T. 15° to 22°). A large absorption of oxygen took place, but it was not replaced by carbonic anhydride; in a second experiment (T. 25° to 26·5°), 20 per cent. of oxygen disappeared, and only 17 per cent. of CO_2 were formed. Pure hydrogen after fourteen days had no action on the bacteria; the gas contained 0·34 per cent. CO_2, 98·94 per cent. H. Pure oxygen after ten days was converted into CO_2 29·98 per cent., O 70·02 per cent. A mixture of CO 46·94 per cent., CO_2 1·27, O 1·27, N 50·51, was next tried after fourteen days; the gas contained CO_2 17·77, CO 0·55, H 7·58, CH_4 2·50, N 71·57. In all of the above cases the bacteria flourished well. Cyanogen was next tried. The solution of meat turned gradually to a thick black fluid. On the fifth day very few bacteria could be seen. From this time, however, they increased, and on the twelfth day were comparatively numerous. On the fifteenth day the gas was analyzed; it contained CN 5·35, CO_2 57·59, O 2·24, N 34·79; a second experiment gave similar results. It appears, therefore, that cyanogen is fatal to bacteria as long as it exists as such, but that it soon decomposes into ammonic oxalate, etc., and that the bacteria then revive, especially in sunlight. Sulphurous anhydride was next tried; the bacteria lived during the fifteen days; the gas contained CO_2 7·87, O 0·00, N 2·13, SO_2 90·10. Similar results were obtained with nitrogen, nitrous oxide, nitric oxide, carbonic anhydride, a mixture of H and O obtained by the electrolysis of water, and coal gas; in all cases the bacteria lived well during the experiment. The author next experimented with a solution of urea (0·98 per cent.) and phosphate of potash (0·4 per cent.) sowing it with bacteria. The bacteria lived well during the fourteen days of the experiment; small quantities of gas were evolved containing 0·53 per cent. CO_2, 2·64 per cent. O, and 96·82 per cent. N. An experiment was made with spongy iron, air, and bacteria. On the fourth day all the bacteria had vanished; the air was analyzed on the fifth day, and consisted of CO_2 0·26, O 0·00, and N 99·74 per cent. Experiments were also made with acetylen, salicylic acid, strychnine (10 per cent.), morphine, narcotine, and brucine; none of these substances had any effect on the bacteria. On the other hand, phenol, spongy iron, alcohol, and potassium permanganate were very destructive to these microscopic growths.

Mr. W. M. Hamlet said that these experiments confirmed some observations of his own. He had found that bacteria could exist in almost anything—in carbonic oxide, hydrogen, 1 per cent. creosote, phenol, methylamin, methylic alcohol, chloroform. Moreover, Crace-Calvert had shown that they could live in strong carbolic acid. In reply to Mr. Waringtou, the speaker said that the acetic acid fermentation went on in the presence of chloroform.

Mr. Kingzett called attention to the fact that the oxygen was completely used up when the meat infusion was placed in contact with air. He did not think the experiments represented the action of bacteria on gases or of gases on bacteria, but rather the effects of various gases on the mode and extent of ordinary putrefaction.

Dr. Frankland expressed his satisfaction with the results obtained by the author in his laborious research. He must confess that these results had surprised him not a little. The fact that bacteria, which were real organisms, and could not be shielded under the term putrefaction, lived and flourished in SO_2, CO, CN, etc., seemed to him very extraordinary, and the question arose whether the germs to which infectious diseases were probably due were not similarly endowed with a power of great resistance to ordinary influences.

Mr. F. J. M. Page said that Dr. Baxter had proved that with some fever-producing liquids, their virulence was destroyed by chlorine and sulphurous acid, and that he had seen some experiments at the Brown Institution which led to the same conclusion; so it seemed that, at all events, in some cases, the virulence of infective liquids was due to organic matter, essentially different from the bacteria observed by Mr. Hatton.

ON THE INFLUENCE OF INTERMITTENT FILTRATION THROUGH SAND AND SPONGY IRON ON ANIMAL AND VEGETABLE MATTERS DISSOLVED IN WATER, AND THE REDUCTION OF NITRATES BY SEWAGE, ETC.

Mr. Hatton then read a second communication. Filtration

* This paper obtained for the author the Frankland Prize of £50 at the Institute of Chemistry.—*Chemical News.*

through sand:—A 14 ft. vertical glass tube, 3½ in. in diameter, was filled with sand. The water was passed through at the rate of 4 liters per day. Experiments were first made with peaty water diluted with its own volume of distilled water. The organic carbon decreased 1·527 parts per 100,000, whereas the organic nitrogen was but little affected. The addition of a nitrifying material, in the shape of 5 c.c. of stale urine added to 4 liters of water, did not promote the oxidation of the organic nitrogen of the peat during filtration. A filtered infusion of rape cake was substituted for the peaty water and similar results were obtained. Some experiments are then given as to the effect of sewage in promoting the reduction of nitrates. A 5 per cent. solution of clear fresh sewage containing no nitrates was added to a solution containing 0·0853 grm. of niter. The mixture was shaken in a large stoppered bottle, and estimations of the nitric nitrogen made from time to time. For a time the nitric nitrogen steadily diminished, until, in fact, the sewage itself began to nitrify, and then the amount increased. At low temperatures the sewage does not seem to nitrify. It was found that when a solution containing nitrates and sewage was allowed to stand in contact with air, the oxygen in the dissolved air increased 4·5 per cent., while that in the air above the liquid decreased 5 per cent. In sixteen days the N as nitrates and nitrites decreased from 0·406 part per 100,000 to 0·075. Thick sewage was much more active than clear sewage. Spongy iron, when shaken up with a solution of niter, converts the nitrogen into ammonia and free nitrogen. Filtration through spongy iron rapidly reduces the nitric nitrogen, converting it for the most part into ammonia. Filtration of peat solution and solution of egg albumen through spongy iron rapidly removed both the organic nitrogen and organic carbon, no nitric nitrogen being formed, all the nitrogen being reduced to ammonia. In some cases the carbon seemed to give rise to some marsh gas.

A NEW CONTINUOUS AMMONIA PROCESS.

A CLEAN and reliable kind of continuous process for the extraction of ammonia from gas liquor is still felt as a want by gas managers, who, with the best desire to do well with their residuals, have little time to devote to superintending a chemical works. The process we are now about to describe claims to fulfill every possible requirement of simplicity, cheapness, and efficiency; and although occupying very little space and demanding the minimum of attention, it is said to extract all the ammonia from ordinary gas liquor down to 1 part in 2,000, or 0·05 per cent. It is, therefore, almost needless to add that it is a lime process; and it is, moreover, in connection with this principle that some of the most striking peculiarities of the apparatus present themselves. It is well known to sulphate manufacturers who use lime in their liquor boilers that considerable inconvenience attends its use in the ordinary way—especially with fire-heated boilers—from the settlement and incrustation of the lime salts on the plates of the boiler, causing great loss of evaporative power and deterioration of the plates of the boiler, and frequently involving a suspension of operations to allow of the removal of the deposit by hand labor. On the other hand, there are considerable drawbacks to the use of steam separately generated for distilling the contents of the lime boiler. The accompanying illustrations of apparatus recently patented by Dr. Hermann Grüneberg show how this gentleman proposes to solve the difficulties of the problem of distillation by fire heat, but without risk of interruption by deposits of lime scale. The external elevation of the still is shown in Fig. 1, as fitted for the extraction of ammonia and its concentration in the liquid form; Fig. 2 shows the same still in vertical section, with adjuncts for the manufacture of ammonium sulphate.

The liquor boiler, A, of cylindrical shape, set vertically, is heated from the furnace, *q*, terminating in the usual circulating flues. The boiler is provided with an inner concentric tube, *a*, which is carried down to some depth below the bottom of the boiler and beyond the action of the fire, and is there finished off inside with a flat perforated screen or sieve, *d*, and underneath with the blow-off cock, *r*. The top of this tube, as will be observed, ends at about the top water-line of the boiler, and is open to the steam space. Over the boiler is set the independent vessel, C, intended to contain milk of lime. The connection between this vessel and the liquor boiler is thus arranged: The two bent pipes, F, F, conduct the ammoniacal steam from the boiling liquor to the bottom of the lime solution, being provided at their lower (sealed) ends with small holes, from which the vapors generated within the boiler are made to issue and agitate the milk of lime. The vapors then rise through the columnar vessel, B, which is a kind of Coffey still, with numerous shallow trays filled with the raw liquor, which is continuously run in by the pipe, L, and in its descent meets the hot vapor and gas on their upward course. In this way the liquor becomes heated as it nears the boiler, and some of its gas is thereby liberated, while the ascending vapors become partly condensed, and mix with the descending liquid, the uncondensed gases passing away by the pipe, *k*. Following now the course of the liquor, by the time it reaches the bottom of the Coffey still, B, much of its volatile ammonia has been driven off; but the remaining liquid, containing those ammoniacal salts which are not volatile, flows by a suitable opening into the before-mentioned lime vessel, C, and mixes with its contents, which are kept in continual agitation by the vapors from the pipes, F, F, as already described. By the action of the lime, the ammoniacal salts become decomposed, the ammonia is liberated, and a portion of it at once ascends through the still, B, and thence finds its exit by the discharge pipe, *k*. The contents of the lime vessel, consisting of liquid yet containing ammonia, find their overflow by means of the connected pipes, *c*, *b*, which are placed concentrically within the boiler-tube, *a*, and terminate at its lower end, just below the screen or perforated plate. It will be observed that the liquid, which, heavily charged with lime, finds its way to the bottom of the tube, *a*, is enabled to deposit the lime and other solid substances beneath the sieve (which prevents them from subsequently rising), where they are quite removed from the action of the fire, and therefore cannot become hardened or burned, and whence they may be blown out at any time by the cock, *r*. Having thus got rid of its solid accompaniments, the liquor finally rises to the top of the pipe, *a*, and overflows into the boiler, contributing its final quota of gas and vapor to the already described circuit, which begins with the pipes, F, F. Thus the operation goes on, the spent liquor being continuously run off from the bottom of the boiler by the pipe, *h*, sealed outside in a deep vessel, J, the depth of which determines the ultimate pressure on the boiler. From this seal-cup the waste liquor is led away in any convenient manner.

This completes the operation as regards the extraction of ammonia from gas liquor. Next comes the consideration of the means for turning it to account. Fig. 2 shows the Grüneberg apparatus as fitted up for the production of sulphate of ammonia. The evolved gas and vapors taken off from the still by the pipe, *k*, pass to the duplicate saturators, K', K''. The vapors and gases from these saturators—which may, of course, be of any approved construction—pass away hot by the large pipe, *u*, and are caused to traverse a cylinder, E, containing a coil of the crude-liquor pipe, leading to the still, the liquid circulating through which is, therefore, warmed by the waste heat of these gases. The uncondensed gases are then finally led to the fireplace to be consumed.

In Fig. 1 is shown a very ingenious arrangement for producing the concentrated aqueous solution of ammonia, which deserves notice. The gas and vapor from the still pass through the cooling pipe, O, which serves as a regulator of the degree of condensation. It is, in reality, a Liebig's condenser, and the more cold water there is supplied to the casing the greater will be the condensation of aqueous vapor in the pipe, and as the condensed water is continually returned to the still, the more concentrated will be the residual fluid which is formed in the second or final condensing coil, D, and thence runs into the vessel, E. The vessel containing this coil is closed, and the condensing liquid is the raw gas liquor passing onward to the still. The uncondensed gas passes from the coil to the seal-box, H, which is provided with an escape pipe. In both figures elevated tanks are shown, which are intended to contain a store of liquor and milk of lime, the supply of both being adjusted as required.

With this apparatus, 1 cwt. of coal for fuel is said to be capable of distilling 1 ton of gas liquor; and two men can attend to an apparatus capable of passing 10 tons of liquor per diem. The apparatus, which is of Continental origin, has been for some time in operation in several large gas-works in different parts of Europe, but has been only lately introduced into this country. It is said to give great satisfaction. The process should be decidedly economical, as the care taken by the inventor to utilize waste heat to the fullest extent is not the least remarkable feature of the design. The illustrations given with this article are taken from actual examples; and the process is simple, although, from its endless character, it requires many words and some repetition of terms to describe in a sufficient manner.—*Journal of Gas Lighting.*

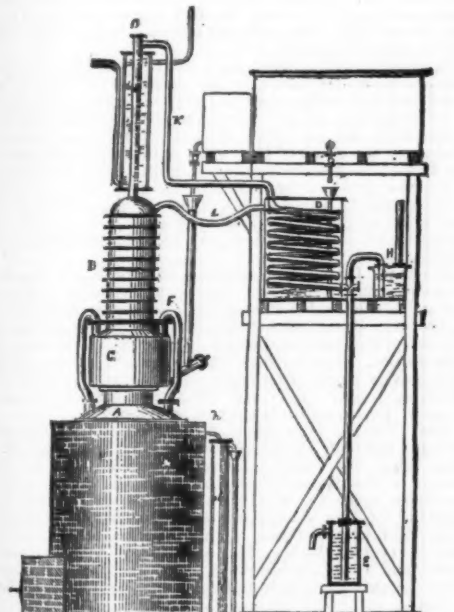


FIG. 1.

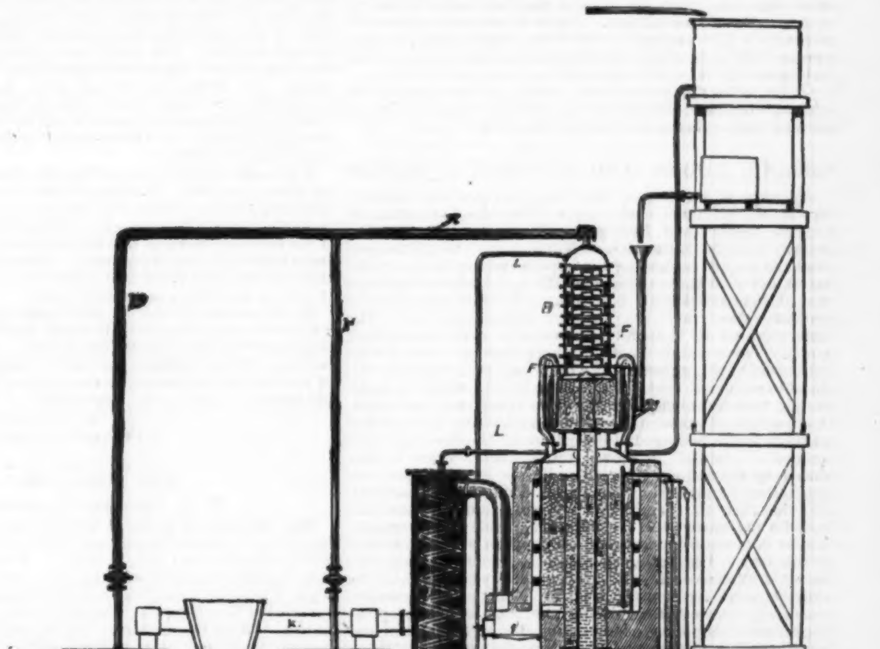


FIG. 2.

NEW CONTINUOUS AMMONIA PROCESS

THE CHEMISTRY OF A LUMP OF CLAY.

CLAY, from the chemist's point of view, is essentially a silicate of alumina, or a salt made up of the three elements, silicon, oxygen, and aluminum. According to the old idea of the composition of salts, it is a compound of silicic acid, which we have in a quite pure form in quartz or "rock crystal," and alumina, which is an oxide of the metal aluminum. It generally contains some uncombined silicic acid, with variable but small quantities of lime, potash, or soda, and of oxide of iron or other metals. It is these admixtures or impurities which give rise to the many varieties of clay.

Clay, as we find it in nature, has been mainly formed by the disintegration of granite and other of the oldest or unstratified rocks containing *felspar* (or *feldspar*, as some prefer to spell it), which is generally called "a double silicate of alumina and potash," though oxide of iron sometimes takes the place of the alumina, and magnesia of the potash. By the long continued action of the air, particularly of the carbonic acid in the air, the rock is gradually crumbled and decomposed. The potash is removed in the form of a carbonate, and the silicate of alumina is separated or washed out by the action of water. This appears to be the general process, though its exact nature is not perfectly understood.

That clay is a very common substance the reader does not need to be told; but that every clay bed is a treasure house of metallic wealth is not so familiar a fact to those who are not students of chemistry. Aluminum, the metal thus locked up, was a chemical curiosity until about twenty-five years ago; and though it has since come to be used to a limited extent in the arts, many of our readers may never have seen a specimen of it. It is a beautiful metal, and would be a most useful one if we could only separate it readily and economically from its compounds; but the key that shall release it from its confinement is yet to be discovered. We can, to be sure, break open the strong doors that shut it from us, but it is a costly undertaking; so that the most abundant of all metals is as yet one of the most expensive, and on that account one of the least useful to us.

Aluminum is nearly as white as silver, and has the advantage over that metal of not being affected by sulphur compounds. It is especially remarkable for its low specific gravity, which is only two and a half times that of water, while silver is ten and a half, or more than four times heavier. It is much the lightest of all the metals that are not affected by the action of the atmosphere. This would make it extremely serviceable for many purposes for which the heavier metals are used, if it were only cheap enough. At present, it is employed only for the manufacture of scientific apparatus in which lightness is an important requisite, and for a limited range of ornamental articles. One of its alloys, however, is extensively used as an imitation of gold. This is the so-called "aluminum bronze," composed of one part of aluminum to nine parts of copper. It combines the strength of iron with the color and general appearance of gold, though it does not retain its luster so well as that metal.

Aluminum must for the present be reckoned among the "coming metals." The practical applications it has received only serve to suggest how useful to man it may hereafter be made; and when we consider how rapidly the nickel industry has been developed within a few years, we cannot doubt that aluminum also has a brilliant future before it. Science will find some easy means of setting it free from its compounds, as it has done with so many other valuable metals.

It is curious that clay has been from time immemorial the familiar symbol of our corporeal integument, and yet this metal aluminum is not one of the elements that enter into the composition of the body. If man was made of the dust of the earth, the dust was not taken from a clay bank. Chemists have indeed sometimes found traces of aluminum in the body, but it is generally regarded as an accidental rather than a necessary ingredient. Hamlet says,

> "Imperial Cæsar, dead and turned to clay,
> Might stop a hole to keep the wind away."

but he could never turn to clay. To whatever "base uses" his dust might come, this plastic application would not be among the number. The "tenement of clay" which poets and rhetoricians make the dwelling of the soul, is built of no such material. "Alexander returneth unto dust; the dust is earth;" but it can never be the "earth" which chemists call alumina, nor any compound thereof.

Quite as curious as this metaphorical misnomer is the fact that clay, though not a constituent of the human body, and incapable of furnishing nutriment to that body, should nevertheless be an article of food with savage tribes in various and widely separated parts of the earth. These clay eaters are found in Western Africa, in the island of Java, among the Himalayas, on the banks of the Orinoco, and in the mountain districts of Bolivia and Peru. In Northern Europe, too, especially in remote parts of Sweden and in Finland, a kind of earth, consisting chiefly of the shells of infusorial animalcula, is much used for food. According to Humboldt, the earth devoured by the Otomacs of the Orinoco region is a true clay, unctuous and nearly tasteless. It is kneaded into balls, from four to six inches in diameter, and baked until the outside becomes reddish—the color being due to a trace of oxide of iron. An Indian will eat about a pound of it daily, the quantity being greater when other kinds of food are scarce, though some is eaten even when fish is abundant. In Bolivia and Peru an unctuous clay is used, which is made into a kind of soup or gruel with the bitter potato of that region.

These edible clays have been analyzed by chemists, and some of them have been found to contain a little organic matter, but for the most part they are absolutely destitute of nutriment. The only purpose that they can serve is to eke out a scanty diet by mere bulk, allaying hunger by their presence in the stomach, "filling" though not nourishing. The appetite for them which leads to their use in connection with other food when the latter is plentiful, must be the result of habit and association. Eaten in moderate quantities, and with a fair amount of nutritious food, clay does not appear to be particularly injurious to health, but excessive indulgence in the argillaceous delicacy may prove fatal.

Of the industrial uses of clay, it was not our purpose to say anything here, though we may refer to some of them at another time.—*Boston Journal of Chemistry.*

ORGANIC CARBON AND NITROGEN IN WATER.

AT a recent meeting of the Chemical Society, London, Mr. M. W. Williams read a paper "On the Estimation of Organic Carbon and Nitrogen in Water Analysis Simultaneously with the Estimation of Nitric Acid." Of all the processes in use for estimating the organic matter in water, the safest and most thoroughly scientific in principle is perhaps that of Frankland and Armstrong. To this process as at present worked there are, however, some objections. The time required to evaporate the water is over twenty four hours. The water is kept for a long time in contact with sulphurous acid, a portion of which may at any time be oxidized to sulphuric acid. There is no test by which to make certain that the nitric acid has been completely destroyed. A correction of some magnitude, calculated by an empirical method, has to be introduced to allow for the dissociation of ammonic sulphite. Moreover, nitrous acid, which is produced by the reduction of the nitrates by the sulphurous acid, attacks ammonia and amidated bodies in acid solution, evolving their nitrogen in the free state, and it is uncertain how far the nitrogen of the ammonia and of the organic matter in a water undergoing evaporation may be attacked in this way. The author proposes to avoid altogether the use of sulphurous acid, and to shorten very considerably the time required for a water analysis. The process consists essentially in converting the nitrates into ammonia by the copper-zinc couple, as described by the author at the previous meeting, distilling off the ammonia with the addition of a little sodium carbonate, and evaporating the residue in the retort to dryness for the combustion. The process may be briefly described as follows: The zinc foil is carefully cleansed from grease, etc., by boiling with dilute caustic alkali, and its surface freed from oxide by washing with acidulated water. It is then immersed in 3 per cent. copper sulphate solution as described in the previous paper. The copper-zinc couple is carefully washed, placed in a wide-mouthed stoppered bottle, and the water poured on, and allowed to digest at the proper temperature until the reduction of the nitric acid is complete. About 1,200 to 1,300 c.c. of water are used. Nitrous acid is present in the liquid as long as any nitric acid remains; 100 c.c. of the water are withdrawn. If a yellow coloration appears in half an hour after adding metaphenylendiamine and sulphuric acid, a longer digestion is needed. If no coloration appears, the reduction is complete. The remainder of the water is poured off from the copper-zinc couple into a tall cylinder, and decanted from any particles of copper and zinc. A liter is distilled in a glass retort, until the distillate is free from ammonia, one or two drops of a strong solution of sodium carbonate being added. The ammoniacal distillate is nesslerized, and, after deducting the quantity of ammonia originally present in the water, gives the quantity of nitric acid present. The water in the retort is further distilled to a low bulk—200 c.c. Any carbonate of lime deposited is brought into solution by the addition of a little sulphurous acid. The water is then rinsed out into a smooth hemispherical basin, and evaporated to dryness in the water bath. The residue thus obtained is free from all compounds of nitrogen, except the organic matters contained in the water. The combustion of the residue is carried out as prescribed by Frankland and Armstrong. The author has employed the process with many waters having nitrates, from 5 to 0·5 part NO_3 in 100,000. The results agree with those obtained by the sulphurous acid method. The author claims for the process that it is free from the sources of error which accompany the use of sulphurous acid for destroying the nitrates, and that it is more rapid.

CHLORAL HYDRATE AND CAMPHOR.—If solid camphor is added to solid chloral hydrate, the two bodies become totally liquefied, and give rise to a colorless syrup, which the authors regard as a true compound.—*P. Cazeneuve and M. Imbert.*

A SIMPLE RAPID FILTERING APPARATUS.

I AM using, in the laboratory of the Massachusetts College of Pharmacy, a very simple method of rapid filtration, for the washing of precipitates, etc., which might be of use to some of the readers of *New Remedies.*

I can, in a very few minutes, with the sharp corner of a file, wet with a saturated solution of camphor in oil of turpentine, bore through the side at the point, A, in Fig. 1, of an ordinary 5 pint or larger packing bottle. I enlarge the hole, with a large round file, to about the size of my little finger; a file wet with the above solution cutting glass as easily as it would very soft brass. Through the hole I force a piece of thick-walled rubber tube about an inch long, and through the bore of this a piece of glass tube with a short end bent at right angle like B in Fig. 1. This makes a water-tight elastic joint with the bottle, by which I connect a piece of rubber tube some six feet long. The bottle, having a funnel fitted into its neck through a tightly-fitted stopple, as shown in Fig. 1, and filled with water, which is allowed to run off through the rubber tube into any receptacle placed below, makes such a simple, inexpensive aspirator as can be improvised in any laboratory in a very few minutes.

FIG. 1.

FIG. 2.

To prevent the bursting of the filter-paper through the pressure, it is supported in the funnel by a tin foil cone. Several of these can be made at a time in the following simple manner: Upon several thicknesses of tin foil is laid a card, having drawn upon it a circle two inches in diameter, like Fig. 2. The circumference, central point, and several lines of radial dots are then pricked through the card and the foil beneath. The circle thus pricked out is then cut out with scissors, the several thicknesses all together. The radius, A C, is then cut through them. Each of the circles of foil can then be formed into a cone by bending the foil carefully round at the central point, C, till the point, A, is just over its opposite point, D. The other half is then carefully folded round outside of this.

This makes it into a cone of double thickness of foil, with an apex of just 60°. In fitting it into the funnel, it can be moulded a little more or less tightly, so as to be made just to fit the funnel, be its angle a little less or more than 60°. A tin foil cone made in this manner serves just as well as a more expensive one of platinum, whenever it is not acted upon by the fluid filtered, or where it is of no consequence if it is, as the filtrate is not to be used.

By the use of this simple, inexpensive piece of apparatus, a filtration is accomplished in about one-fiftieth of the time consumed with the simple funnel alone; the exact gain depending, of course, upon the relative length of the column of fluid in this apparatus, as compared with that in the simple funnel. Yours, very respectfully,

V. F. DAVENPORT, M.D.,
Professor of Analytical Chemistry.
—*New Remedies.*

THE LIME-LIGHT.

By T. FREDERICK HARDWICH.*

MR. HARDWICH prefaced his remarks by stating that the best of the nipples for ordinary work was undoubtedly that with an aperture of 1-20th of an inch. It was a larger size than that usually supplied with lanterns, and, said Mr. Hardwich, I adopted it originally on the recommendation of Mr. Cooper, of Northampton, who saw it first, I believe, in Edinburgh. With this orifice to the burner, one and a quarter pounds of chlorate of potash will supply enough gas for an hour and three-quarters; the pressure on the bags being three-quarters cwt., and the oxygen tap turned fully on. The quantity of coal gas used is not much greater than that of the oxygen, but I still adhere to the opinion I originally expressed, that if an extra weight be required on this bag, there is not the slightest danger in putting it on, since the pressure is equal in front of the taps where the gases mix, although unequal on the bags.

The angle at which the flame impinges on the line I do not find to be of the importance I anticipated; indeed, I obtained a most excellent light by doing away with the angle altogether, and shooting the flame along almost parallel to the face of the cylinder. It would not be possible, however, to work in this way, as the point of the jet is so near that you cannot rotate the line.

On the whole, I am inclined to give the preference to the angle recommended by Mr. Newton, of Fleet Street, London, in the discussion on my paper, viz., as near to a right angle as is possible without throwing a shadow on the condenser —say 45°; the advantage is that the light is somewhat whiter, and the jet can be placed at a greater distance from the lime. With the smaller angle of 17° you obtain a very strong light, bringing out all the details in the shadows of the picture, but there is a tendency to yellowness, which a practiced eye at once detects. I explain this by the fact that the lime spot, although larger, and consequently giving more light as regards quantity, is not so intensely heated; this is shown by its wearing away more slowly under the action of the flame.

Besides being whiter, the wide angle of 50° to 55° has another obvious advantage: it admits of the jet being removed to a greater distance, so that, if the lime should crack, or be improperly centered, it will still rotate without touching the platinum point. About one-eighth of an inch I find to be a good striking distance; but it must be borne in mind that the cylinder expands when heated, and approaches a little nearer to the point.

In the jet sent to me by Mr. Oakley, the lime cylinder is upright; but in some of the older forms made a few years ago, the lime was inclined toward the condenser. One wonders why this should have been done, as the other plan is more simple and convenient; but I do not find that it makes any appreciable difference in the light, and hence it will not be worth while for those who had the old-fashioned instruments to put themselves to the expense of getting them converted.

I used the expression "*if the lime cylinder should be improperly centered.*" This, I am sorry to say, is often the case, and the attention of the manufacturers ought to be directed to it. Very frequently, too, the hole is so choked up by lime dust, that you lose the center in boring it afresh; this may be avoided by stringing the cylinders into a chain, as is sometimes done. The bore of the lime is also too large for the pin of the jet, in many instances sufficiently so to make a difference of an eighth of an inch in the distance during rotation. To overcome this, take a piece of straight wire of the same size as the pin, and roll round it a strip of writing paper, about three inches long and half-an-inch wide, then put on the cylinder and transfer it to its proper position on the jet. All this may seem troublesome, but it takes less time to do than to describe, and is a great help in keeping the light steady. A still better plan is to use thin sheet lead instead of paper, and to leave it permanently attached to the pin.

Having examined jets by the best makers, both in London and Birmingham, I can give the following directions for testing: First, examine whether the jet is *gas tight;* close the orifice of the nipple tightly with the forefinger, and turn off one of the taps, then suck at the other tap, and notice whether the tongue is tightly held for a minute or longer. If it is not, there is a leak somewhere, most probably at the nipple screw, and a small washer must be made of two or three thicknesses of the sheet lead used for wrapping tea. Next try the lime carrying pin, whether it is straight and also vertical; bring the point of it close up to the orifice of the jet, without any lime, and turn it slowly round; you will then see at once whether it is right; if not, it must be straightened, or you will lose in steadiness of light. Lastly, test for smoothness of bore; the absence of this will be shown by a *humming* noise, becoming more pronounced when the hydrogen is in excess: and the cure will be to remove all sharp edges internally, by a proper tool, especially those near to the orifice of the jet.

The flame of the oxyhydrogen burner should be noiseless, and if it is not so there will be a little loss of light. I have read of an experiment in which the nozzle of a fire-engine was carefully polished in the interior; the stream of water came with great force, and was clear and transparent as glass. The inside was then purposely scratched and roughened, the effect of which was that the column of water was now opaque and troubled, and the height to which it rose fifty feet less. Of course the two cases are not precisely analogous, the velocity being so much greater in the latter; but I do not think that sufficient attention is paid to smoothing the bore of these gas jets.

In working with a small angle of 17° or 20°, the lime appears to be heated mostly by the outside of the flame, and a small excess of coal-gas does not interfere; but when the angle is 50° or greater, the two gases must be in exactly the right proportions, and an excess of coal-gas will cause a dark nucleus in the center of the spot. I find it best to regulate the flow of the gases by looking at the line itself, rather than at the disk; and as it can be done more easily when the *point* of the flame is visible, I work at the top edge of the lime, and, if the cylinder is too long to admit of this, I cut off the top part with a small fret-saw, such as most schoolboys now possess.

It remains for me, in conclusion, to make a few remarks on the precautions necessary to prevent explosion. At the meeting of the London Photographic Society, before referred to, fears were expressed that the use of the oxyhydrogen jet by any except the most experienced operators would be attended with danger. It seems to me that any person accustomed to photography ought to be able to use this jet with safety by observing proper rules; and without such rules even the "safety" jet, so-called, is not free from danger.

First. The two bags should be kept separate, each for its own gas, and both should be emptied after the lecture, the taps being left open. I have been told that in the case of a clergyman alluded to in the discussion, where the whole apparatus was blown to pieces immediately on applying the light, a bag partly full of oxygen had been sent to be filled with coal gas. If the rule had been followed always to empty the bags, this accident could not have happened, and every chemist knows that the bags last longer when the gases are squeezed out at the expiration of the lecture.

Secondly. A weight once placed on either of the bags should not be removed without first extinguishing the light; the expansion of the gas would be likely to cause a suction of the flame backward, and cases are reported of an explosion even in the oxygen bag from that cause.

The following experiment has been mentioned to me since I read my last paper. A long oblong box was made, and glazed with windows at the sides; the floor of the box was then strewed with fine *coal dust*, and a pistol shot was fired in the inside. There were two reports—first that of the pistol, and next an explosion of the coal dust, which blew out the glass of the windows. Now, if coal dust will burn so rapidly in *air* under some circumstances as to produce an explosion, how much more may any finely-divided combustible powder be expected to burn in *oxygen;* and we know that old oxygen bags often contain a white powdery substance in considerable quantities.

In my own practice I always look upon the oxygen bag as a source of at least equal danger with the hydrogen; and I appoint a trustworthy person at the commencement of the lecture to watch both bags, and see that no one comes near, either to prick them with pins, or lean upon them with the elbow. With those precautions I have not had the smallest accident in the course of several years.

Mr. Frederick York, of Notting Hill, London, has lately sent me two samples of chlorate of potash, marked respectively, "commercial," and "pure, at double the price." The only difference I could detect was that the gas from the commercial had a slight smell of chlorine on issuing from the retort, while that from the pure had none. The "commercial" gas had no smell after passing through the second purifier containing solution of carbonate of soda. The quantity of the gas was nearly the same in both cases, and the light was the same as far as I could judge. I infer, therefore, that good commercial chlorate of potash is sufficiently pure for lantern work, and that the expense attending repeated recrystallization may be avoided as unnecessary.

* Read before the Edinburgh Photographic Society.

HYDROBROMIC ACID AS A REAGENT FOR COPPER.—A drop of the solution in question is placed in a watch glass, a drop of hydrobromic acid is added, and the mixture evaporated at a gentle heat. When it is reduced to the bulk of one drop a rose red coloration appears, three or four times more intense than that produced by potassium ferrocyanide. In this manner one one-hundredth milligrm. copper may be detected.

PREVENTION OF DIPHTHERIA.

By EDWIN R. MAXSON, A.M., M.D. LL.D., of Syracuse, N. Y.

SINCE diphtheria originated in Egypt, near 2,500 years ago where it prevailed, and in Asia Minor, 500 years before extending further, and hence was first called *Egyptian* and then *Syriac disease*, the question as to its *prevention* has often been asked, but perhaps never quite satisfactorily answered.

And yet when the facts are carefully examined, taking into account its *history*, *causes*, and *pathology*, there is nothing strange connected with the inquiry. A glance, then, at the subject, in a general way, on this basis, may enable us to arrive at a rational conclusion as to its *prevention*, which should be of present and future benefit to the human family.

That diphtheria, a general putrid febrile affection, so nearly allied to the plague, an offshoot of which it very very likely is, should have originated, as well as the plague itself, in Egypt, from putrid animal and vegetable emanations arising from the drying up of marshes or pools of water in the old cemeteries, after the yearly overflowing of the Nile, is precisely as might have been expected; and especially when we take into account the various imprudent habits prevalent among the Egyptians during the Assyrian and Ethiopic invasion and control of the country,* including numerous deviations from the laws of health and rules of propriety, many of which, with others added to the list, there and elsewhere, have been predisposing to the disease, and accounting for its spread and prevalence down to the present time.

For, though the poisonous effluvia arising from decaying animal and vegetable matters along the valley of the Nile may very likely have been the first cause of the disease, it should be remembered that the *poison*, or "bacteria," first operated upon constitutions that had been rendered imperfect, not only by their own imprudence, but also by the indiscretions of their progenitors. And the degree of physical degeneracy thus produced, together with the effect of climate, etc., in different localities, constituted the difference of predisposition to the disease. And hence it was, no doubt, that diphtheria extended first into Asia Minor; where, it is likely, that all the circumstances contributed to constitute the strongest predisposition, and especially the invasion and wars of the Persians.†

And the same holds true in relation to its extension, after 500 years, into the south of Europe; many degradations connected with the decline of the Roman Empire, and especially the wars with the Northern barbarians, as well as the Crusades, etc., having contributed to its extension, during the subsequent 1,500 years, across the continent of Europe, the corruptions of the dark ages having contributed to increase the predisposition to the disease and its spread.

Thus we find that in this extension, prevailing mostly in garrisoned towns, it became epidemic in Holland in A.D. 1337, in Paris in 1596, and in America in 1771,‡ having since prevailed extensively in France in 1818 and 1835, and in England and the United States, from 1856 to the present time. And though the disease had originated in Egypt, no doubt, as stated, and had extended, like other contagious febrile affections, in the direction most predisposing, taking all the circumstances into account, diphtheria, like all other contagious diseases, and more than many of them, as often arises spontaneously, among the predisposed, from *putrid animal* and *vegetable* substances, so that contagion alone is by no means the sole cause of the disease.

And while too much care cannot be taken to avoid exposure to the *contagion* of diphtheria, it is of as much importance, as a means of exterminating it, and of vastly more importance, as a direct prevention, to remove the physical predisposition and general exciting causes of the disease as they now prevail

The causes of diphtheria, predisposing and exciting, besides contagion, may be regarded as embracing every possible deviation from the laws of health and rules of propriety, not only of the children, who are the more common victims of it, but also of their ancestors. For many children are hereditarily predisposed to this disease from the effects of diseased parents, the result of various imprudencies, as the use of tobacco, intoxicating drinks to excess, licentious habits, improper food, and irregular eating; and, in short, every deviation of the parents or their progenitors from the proper rules of life, physical, intellectual, and moral.

Many children also become predisposed to diphtheria by irregular feeding in infancy, improper clothing, such as short sleeves, short pants, etc., and irregularity in taking food, poisoned candies, and various unwholesome and indigestible articles of food and drink during childhood; and later, improper food and unwholesome drinks, late suppers, tobacco, etc. All these and many other kindred improprieties are allowed by too many parents, tending to impair digestion, derange the circulation, and interrupt nutrition, leading to an enfeebled state of vital energy, and hence predisposing to diphtheria.

Now add to all these influences *contagion*, and the various exposures of children to sewer gas, filthy, damp apartments, water-closets and filth in back yards, around barns, and in the streets, as well as the too frequent use of impure water, and we are left to wonder that so many escape the disease at all, or that so many recover, having contracted the disease, as usually do.

Diphtheria is a general putrid, contagious febrile affection, more nearly allied perhaps to the *plague* than any other disease, and whether originating in contagion or from putrid animal and vegetable decaying matters, may have an incubation of a week or longer. Like small-pox, it may be produced by the inhalation of an impalpable effluvium, or by palpable matter; in the one case developing the general febrile state, before there is any local manifestation of the disease, while in the other there is first a local disease, and then a general febrile state developed; and the *manner* of the introduction of the poison into the system appears to modify the disease, precisely as in the case of small-pox as contracted by the effluvium or inoculation.

The poison of diphtheria, whether from contagion or putrid exhalations and however introduced into the system, dissolves, more or less, the blood, rendering it unfit to sustain the nervous system; thus undermining the various functions of the body, and the partially dissolved fibrin and albumen become a more or less putrid, feebly organized fibrino-albuminous exudation membrane, on the surface of the fauces, larynx, trachea, bronchiæ, and sometimes on the lining membrane of the heart, arteries, veins, and alimentary canal, one entire cast of which, measuring nearly a pint, having fallen under my observation in a boy that recovered.

Death may result from the mechanical effect of this membrane in the fauces, larynx, trachea, or bronchiæ, by interrupting respiration, hindering decarbonization of the blood, or it may occur from the direct depravity of the blood, failing to sustain vitality, or, again, by an exudation into or upon the brain and spinal cord or their membranes, producing mechanical pressure, and hence paralysis of the voluntary and often of the vital functions. Death may, however, occur from a combination of these various conditions, attended with general derangement before the suspension of the gastric, hepatic, and renal functions, *uræmic* poisoning sometimes suspending vitality.

Hence it is that all depressing influences, including every deviation from the laws of health, may not only predispose to this disease, but will, in every case, greatly increase the danger of a fatal termination when once contracted.

PREVENTION.—To prevent diphtheria, then, and so finally exterminate it, every man, woman, and child, throughout our land and the world should be brought to obey the laws of life and health.

Parents should regularly feed, properly clothe, and duly restrain all children, before they come to the years of understanding and accountability. This alone would do much. A late prominent physician of Paris estimated that 3,000 children had died in that city, during the thirty years of his practice there, from short sleeves, short pants, and other kindred imprudence in the dressing of children. And I am fully convinced that as large a proportion are sacrificed, in towns at least, in this country, from the same cause—all for a *wicked* fashion. And from careful observation in this country and abroad, I am confident that at least as many more are carried off by improper food and irregularity in taking it, together with poisonous candies and other unwholesome and indigestible trash, that no child or other person should eat.

Many of these, it is true, do not die of diphtheria. But it should be remembered that all this goes to predispose those not actually killed by depraving the blood and lessening the powers of vital resistance. And hence, when exposed to the *contagion* of diphtheria, or to putrid animal and vegetable exhalations, they are the first to take and most liable to die of it.

Children on attaining the age of accountability, and all other persons, should take plain, nourishing, and digestible food, with strict regularity, and nothing between meals or late at night. Trash, tobacco, intoxicating drinks, cosmetics, hair dyes, dime novels, etc., should be avoided by all. And while the amount of clothing should not be in excess, care should be taken to keep the arms, legs, and feet well protected, and all dress should be adapted to the season

The person should be kept *clean*, without too much fretting of the skin by unnecessary washing, lest the urinary or other excretions should be called to the surface, thereby increasing personal filth, and injuriously deranging the various functions of the body. Sleeping rooms should be as far from the ground as possible; water should not be allowed in cellars, for a day even; and no decaying vegetables should be kept there.

Pure air should be allowed to pass into, and foul air out of, sleeping and all other rooms, without admitting dampness, or exposing the occupants to chilly night air more than can be helped.

No stagnant water should be allowed about a dwelling. And the back yards, where children play, should be kept exquisitely clean. Drains for sinks should be kept in order; and privy vaults should be cleaned out as often as twice a year, lime being thrown in at least once a week, and if convenient dry earth each day.

Heaps of filth should never be allowed about barns or other out-houses. Hencoops, pig-sties, and rabbits' cages, if allowed, should be as far from the house as possible, and kept exquisitely clean; and no water should be used that could possibly contain decaying animal and vegetable matter.

Children should not be put to such kinds of labor as would expose them to injuries from filth, damp air, or other injurious influences. And adults should avoid such exposures as far as possible.

Now, as it was a deviation from all these rules of propriety which has predisposed to and kept up diphtheria, and all other kindred diseases, it is only by a return to these laws of health and rules of propriety, in every minute particular, that they are to be prevented and exterminated. And, while all this cannot be accomplished at once, very much can be done *now*, and more ultimately by getting right in all these particulars. And public hygiene, carrying out these principles in ample drainage, supplying pure water, cleaning streets, and suppressing all nuisances, may aid greatly in this work. And when a generation shall have been raised up with such habits and without the hereditary predisposition to this and other kindred diseases, children not inheriting it, properly cared for by their parents and obeying all the laws of life and health, may become in a great degree secure from the ravages of diphtheria, and hence of other putrid diseases. And as the body is the instrument of the mind, physical disease may not only be eradicated in the main, but the intellectual and moral powers of mankind will become proportionally elevated; and thus humanity may in a measure approximate Divinity, and become more nearly "allied to angels on the better side." Let us labor for this, then, as not only involving the *physical*, but also in an equal degree, the *intellectual and moral well-being of mankind*.—*Sanitarian*.

* Circa, from B.C. 672 to 600.

† Circa, B.C. 560 to 546.

‡ Transactions Philosophical Society, vol. 1.

PROLIFIC PEOPLE.

MICHAEL HAZZARD, of Monticello, Pratt County, Ill., has sent to the Washington *Republican* a picture of five babies borne by his wife on the 18th of September, 1880, and whose combined weight was 19½ pounds. Hazzard is 39 and his wife 36 years of age. The *Republican* says: The prolific powers of some individuals among mankind are very extraordinary. Instances have been found where children to the number of six, seven, eight, nine, and sometimes sixteen have been brought forth at one birth. The wife of Emmanuel Gago, a laborer, near Valladolid, was delivered on the 14th of June, 1799, of five girls. The celebrated Tarsin was brought to bed in the seventh month at Argenteuil, near Paris, 17th of July, 1779, of three boys, each 14¼ inches long, and of a girl 13 inches. They were all baptized, but did not live twenty-four hours. In June, 1799, one Maria Luiz, of Lucena, in Andalusia, was successively delivered of sixteen boys, without any girls. Seven of them were alive on the 16th of August following.

In 1535 a Muscovite peasant, named James Kyrloff, and his wife were presented to the Empress of Russia. This peasant had been twice married, and was then 70 years of age. His first wife was brought to bed twenty-one times—namely, four times of four children each time, seven times of three, and ten times of two, in all fifty-seven children, all then alive. His second wife, who accompanied him, had been delivered seven times—once of three children, and six times of twins. Thus he had seventy-two children by his two marriages.

SPECTRUM ANALYSIS AS APPLIED TO THE SOLAR SYSTEM.

A LECTURE entitled "The Chief Results of Spectrum Analysis as Applied to the Heavenly Bodies," was lately delivered at Trinity College, Mandeville place, Manchester square, by Dr. Wm. Huggins, Esq., D.C.L., LL.D., F.R.S., etc.

Dr. Huggins said: I shall have to ask you to night to listen to the music of the spheres. It is from the songs of the heavenly bodies that the astronomer now is able to obtain much information as to the nature and history of these bodies, which but a few years ago appeared to be hopelessly and for ever beyond his reach. But the music to which I shall have to ask your attention is the music of the eye, and not the music of the ear. The rhythmic vibrations in which the stars speak to us take place in ether, and not in air, and therefore they have to reach our consciousness through the sense of sight, and not through the sense of hearing. But though the eye is most exquisitely sensitive to light, it does not possess the power which the ear has of separating and distinguishing the several elements of a compound sensation. The musician can distinguish the several component notes when a chord is struck, and a trained ear would be even able to analyze the intensely compound rush of sound from an orchestra; but in this particular the eye fails us. We are not able to distinguish the several kinds of light which fall together upon the eye unless we bring to its aid the use of the prism. Spectrum analysis, which rests on the use of the prism, may be said almost to have furnished us with a new sense, since it enables us to study separately each of the different kinds of light, which ordinarily are lost in a common sensation.

Now, when light falls under suitable conditions upon a prism, of glass, or any other transparent substance, the light is turned round and diverted from its original course. But it is not bent as a whole. All the different kinds of light which exist in the compound light which falls upon the eye, are bent differently the one from the other. The consequence is that they all take different paths. The obvious result follows that they part company, and thus they, so to speak, arrange themselves in single file and light, when thus analyzed and presented to us, is under the conditions of what we call the spectrum of the light. When we examine the light from different sources, we come at once face to face with spectra of different kinds. If the light comes from highly-heated solid, or liquid, or gaseous matter in a very dense state, then, as a rule, but not necessarily and always, this light, when passed through a prism, presents a spectrum in which there is an unbroken range of all the colors. It is a chromatic scale of light with every possible interval. If, however, the light comes from heated matter in the state of gas, then we have quite a different state of things. We have a series, so to speak, of notes of light separated from each other. It may be that these notes represent a fundamental and upper partials. Whether or not these lines have any harmonic relation, each body gives out a set of lines peculiar to itself. It sings its own song, and the substance can be easily recognized by its particular set of lines.

If the body which becomes luminous is a compound body, and can become luminous without suffering decomposition, the set of lines is then that which distinguishes the compound; but if the compound can suffer decomposition, then we have together a series of sets of lines, each set corresponding to each component existing in the compound body. But when we look at the songs of most of the heavenly bodies, we have a spectrum which is neither this spectrum nor that, but a spectrum in which the range of colors is interrupted by a number of dark lines, which are narrow spaces where there is no light. If two harps are strung in tune the one with the other, and then placed at a distance apart, if a chord be struck on one, the other harp will murmur forth the same notes. This depends upon the fact that a string takes up motion very readily from waves of air which have a period the same as that which the string would produce in the air if it were made to vibrate. But as nothing can come out of nothing, it is obvious that the sound produces this murmur in the harp at the expense of its own life; just in proportion as the motion of the air is taken up by the string it ceases to exist in the air. This is a very fair analogy of what takes place among the molecules of the gas.

These molecules of the gas absorb and take up the vibrations of the ether, which are of the same period which these molecules would excite in the ether if they had been stimulated into greater activity by heat. Hence these dark lines are produced by the absorption of different substances, and these dark lines occupy exactly the same space in the spectrum that the bright lines would occupy. And consequently, comparing the dark lines with the bright lines of any terrestrial substance—any substance rendered luminous artificially—we are able to determine whether these terrestrial substances do or do not exist in the sun or any of those heavenly bodies to which our instruments are directed. Having thus briefly touched on the method of spectrum analysis, I shall now bring before you rapidly some of the principal results, confining myself chiefly to those results which have been obtained in my own observatory. It was long ago demonstrated by Kirchhoff, Bunsen, and others, as the result of laborious investigations and comparisons of spectra with the solar spectrum, that many terrestrial substances exist in the atmosphere of the sun; and this, indeed, was to be expected because of the obvious common origin of the sun and the planets, indeed of the almost certain original oneness of the stuff out of which they arose. But when we come to the stars, here the problem stands altogether upon another footing.

If we realize for a moment the enormous distance of the stars from us, we shall see that there is no presumption as to a common origin or a common material; and there is nothing whatever to tell us whether in the stars we should find the same state of things that we find here in the solar system; whether we should find the same physical substances, the same physical laws, or even the same geometric relations. And it is worth while for a moment to consider the enormous distance of the stars. The earth's orbit, which is more than 190,000,000 of miles in diameter, at most of the stars dwindles to a point, and has no sensible size whatever. Rather an ingenious illustration has recently been given by Professor Ball, of Dublin. If you suppose, he says, a railway from the earth to the nearest fixed star, which is supposed to be twenty billions of miles from us; and if you sup-

pose the price of the fare to be one penny for every hundred miles—not, mind! a penny per mile—then, if you take a mass of gold to the ticket-office equal to the national debt, it would not be sufficient to pay for a ticket to the nearest fixed star And I think I may go further even than Professor Ball. I think I should not be wrong in saying that there are stars so far off that at the price of one penny for every hundred miles, the whole treasure of the earth would not be sufficient to pay for a ticket. You see, therefore, that the statements which have been made as to the nature of these bright points which we see can be nothing more than the guesses of men according to their prejudices or their predictions. This method of analysis has, however, enabled us to obtain certain information as to a great deal of what is going on in the stars.

The room was then darkened, and a number of slides were thrown upon a screen. The first showed the interior of the lecturer's former observatory, together with the equatorial telescope, which continued to follow the course of the star by a clockwork arrangement. The spectrum was placed at the hinder end of the telescope, and there was an arrangement by which artificial spectra could be compared simultaneously with the spectra of the stars. The next slide showed the results of the analysis of two stars, indicating that the spectra of stars presented the general features of the solar spectrum—a continuous band of colored light crossed by spaces where there was no light, indicating the absorption of vapors in the stars The presence of various chemical elements in the stars had thus been ascertained. It was not necessary for his present purposes to go into detail as to the precise elements. The point of interest was, that matter similar to that which existed in the earth existed also in the stars.

It was rather an interesting point to find that some of the substances which were most widely distributed in the earth, and which were essential to life as it existed here, such as hydrogen, magnesium, and iron, were the three elements of which we had the strongest evidence of a wide distribution among the stars. Dr. Huggins pointed also to the spectrum of another star, which he said was of a slightly different type, and in which was to be seen the commencement of shaded bands or lines. There was good reason, he said, for believing that this arrangement indicated a rather lower temperature of the surrounding atmosphere, and might also indicate the presence of compound bodies. Another slide Mr. Huggins referred to as showing the spectrum which was peculiar to the great class of white stars, such as Sirius. The peculiarity consisted essentially of four very strong lines. These four lines were the lines of hydrogen, showing that hydrogen existed in very large quantities in the atmosphere of these stars.

In addition to these, there were a number of very fine lines, which were so very fine that they were only seen under circumstances of extreme clearness in the atmosphere; so that ordinarily the spectrum appeared to consist of the continuous spectrum crossed by these four strong lines of hydrogen. Mr. Huggins next caused to be exhibited a spectrum of a slightly different order, in which there was a still greater approach to the condition of shaded bands. This was the spectrum of the bright star of Alpha Herculis. Here there is a different arrangement of light; this star showing also the presence of many terrestrial elements, but indicating different conditions of temperature and pressure in the surrounding atmosphere Other slides were also exhibited, showing how the spectra became more shaded and darker as we approached the stars of the red order. Dr. Huggins continued as follows: The spectra that we have considered are, after all, only a small portion of the true spectrum of the stars.

What we have been reading is merely, as it were, a little piece out of the middle of a page of the writing which is contained in their light. The other ends of this writing are written in sympathetic ink, and are invisible to us. We cannot see them unless we adopt some method by which we can render the other portion of the writing visible to us. This may be explained in this diagram. The whole length of this diagram represents, very inadequately indeed, the radiations from a highly heated body. But this small part in the middle represents so much only of it as the eye is capable of receiving. Below the red, there are long waves of invisible light which fall upon the eye, and of which we are not conscious; and so, again, beyond the violet, there is a very long series of higher and higher waves, shorter and shorter vibrations of light, which do not affect our consciousness at all. But we are able to become acquainted with this portion of the spectrum through the action of these shorter waves upon various substances. One of these methods is the common method of photography. These shorter waves, which are unable to affect the photographic layer of the eye, are able to affect an ordinary photographic plate. They are able to decompose the delicately balanced silver salts which are on the photographic plate. Therefore, it was exceedingly important to supplement these eye observations of the stars by photographs of the spectra of the stars. It will be seen at once how extremely difficult a task this was, because of the faintness of the light of the stars as well as on account of their motion; because to photograph the star itself is not too easy, and to photograph its spectrum it is necessary that the light of the stars shall pass through a train of prisms. In the first instance, the image of the star shall fall upon a slit not wider than about the 350th part of an inch. After passing through the prisms, this light is spread out into a spectrum of about an inch in length. Therefore, each part of this spectrum is only about 1-400th part of the brilliancy of the image of the star. You will see, therefore, how difficult it is to obtain a photograph under such circumstances. Bearing in mind, too, that on account of the rotation of the earth, the stars appear to be constantly moving, and it is therefore difficult to retain the light of a star on a slit the 350th part of an inch in width. This, however, we have been able to accomplish, and I represent in this diagram some of the results of these photographs. I have not time to go into the grounds for the statement, but there are good reasons for believing that this arrangement of stars shows the order of their development—that is to say, the relative ages of the stars. Dr. Huggins again proceeded to illustrate his lecture by causing slides to be thrown upon a screen.

A remarkable phenomenon was to be seen, he said, when the stars were watched, namely, the increase and diminution of their light. There were probably very few stars which were absolutely invariable in their degree of brightness, and these changes occurred for the most part according to a fixed law of periodic variation. Up to this time the spectrum analysis has not thrown any conclusive light upon the cause of these variable stars. But there was the occasional phenomenon of what was called the outburst of a new star, which was for good reasons believed rather to be an extreme case of variability, as most of the new stars did not die out altogether. They returned to an exceedingly faint condition; and it might be, although we had not sufficient knowledge of this at present, that at a future period another outburst might take place. So far as was known, no new star had been added to the heavens. Some few years ago there was a very remarkable case of this kind. A bright star suddenly appeared in the Northern Crown. Its spectrum was immediately examined, and there were seen, in addition to the ordinary star spectrum of dark lines, a series of brilliant lines, and on comparing these bright lines with terrestrial substances, it was found that they were undoubtedly due to hydrogen, showing the existence of a very large quantity of hydrogen in a luminous state about this star. This star continued to have great brilliancy for a few days only, and then it returned very rapidly to its former state of insignificance. It now exists as a star of only about the eleventh magnitude, and was visible only through very large telescopes. We could not exactly explain the state of things which resulted in this sudden outburst of brilliancy. Nevertheless, we could hardly adopt the view of an old astronomer, who supposed that stars of this character were bright only on one side and dark on the other, and that every now and then the Deity was seized with the caprice of turning them round. (Much laughter.) Dr. Huggins went on to say that there were other objects in the heavens beside the stars. The telescope revealed a number of objects of extraordinary form and size. With one or two exceptions, the so-called nebulæ were not visible to the naked eye in this hemisphere.

A few years ago, when this science of spectrum analysis first arose, and before it was applied to these objects, the prevailing opinion seemed to be that Sir William Herschel had, perhaps, been mistaken in supposing this nebulous matter to be the stellar protoplasm out of which the hosts of heaven were fashioned. The rapid increase in the power of telescopes, and especially the great telescope erected by Lord Rosse, seemed to lead to the conclusion that all these bodies could be broken up into points of light, and that they were not truly nebulous or gaseous in their nature, but consisted of congeries or masses of stars, whose individual light was lost in a common blaze. Mr. Huggins went on to speak of the lines in the nebulæ, and of the remarkable forms which these bodies assumed. There was no doubt that matter really did exist in the heavens in a very different state from the ordinary stars. There might be great masses of gaseous matter, or in some cases great systems composed of separate gaseous masses, with more or less central condensation—it might be, indeed, incipient suns; but many points have to be considered. But there were some nebulæ which had been clearly resolved, and presented quite a different spectrum. Among them was the nebula in Andromeda. But whether or not this was the true star protoplasm, the original stuff out of which the stars that are seen have been elaborated, was a point upon which there was no certain knowledge. But there was this difficulty. We should have expected to find a series of spectra passing from the most simple form of two or three lines to a greatly increased number and complexity of lines, until we got the fixed star. Such a series, however, did not exist. The nebulæ might be divided sharply into two divisions—those which gave a four-line spectrum and those which gave a continuous spectrum, where the light is so faint that it was impossible to say whether dark bands existed there or not. There were also other bodies in the heavens, namely, the comets. These had from all time presented many difficulties, and had been, and were still to some extent, a great riddle to astronomers. He would, no doubt, have been able to speak very positively this evening of the nature and conditions of comets, if a large comet had appeared to us within a period during which the spectroscope had been in the hands of the astronomer; but, unfortunately, since the astronomer had been able to bring this great arm of precision to bear upon the heavens no great comet had ventured to show itself. (Laughter.) There had been nothing but poor miserable wisps of things, which had been invisible to the naked eye. The information that had been gleaned in regard to these bodies, though very considerable, necessarily fell far short of the information which at once awaited us if only a comet of great brilliancy should come before us.

Turning to a slide representing the different parts of a comet, Dr. Huggins pointed out the bright point or nucleus, in front of which, he said, there was usually alternate bright and dark portions, and the tail of the comet consisted of matter driven off in a direction opposite to the sun. It had been considered a moot point, before the application of the prism, whether they possessed any light of their own. When the prism was applied, it was seen at once that it was not solar light with which we had to do, but matter containing carbon, possibly in combination with hydrogen. The nucleus itself gave a very bright continuous spectrum. There was one other very important piece of knowledge which the spectrum analysis had given to us—information which a very few years ago it would not have been considered within the power of man to obtain. It was proved that nearly all the stars had a proper motion of their own. He was not speaking now of such apparent motion from the motion of the solar system, but of true motion. This proper motion was determined by watching the motion of the stars with relation to other stars close by It was quite obvious to every one that if the motion were in the line of sight we should not be able to perceive it at all. Just in the same way, if we stood exactly in the line of a train it would not appear to move at all It was believed to be quite impossible to obtain a knowledge of their motion in that line of sight; but if it could be discovered it would contribute a great deal towards a knowledge of the mechanism of the heavens, and the understanding of the relations of individual stars to groups and constellations. This was a matter which had been done. As long ago as 1840 Doppler suggested that, like as the sound of a musical instrument increased or decreased in pitch as it approached or receded, so it might be that a star might alter its color if it were rapidly approaching the earth or rapidly moving away from it. In the case of musical sounds the experiment had been attended with certain results. Anybody standing at a railway station had only to notice the pitch of the whistle as the train approached and as it receded. The pitch of the whistle would be enhanced in proportion to the approach of the train. When the train had passed the station the whistle would be lower in pitch in exactly the same proportion.

Doppler's suggestion that a star would change its color in consequence of motion was not true as he suggested it, but, nevertheless, it was true in a sense; and observations of the principle he had pointed out showed in certain stars a motion equal to about forty miles per second. He (Dr. Huggins) had applied this method to about thirty stars, and this system of observation had since been taken up at Greenwich, and was now part of the regular work of the Greenwich Observatory. The results of the method of observation by spectrum analysis formed too wide a subject to enter into fully. At present, laborious investigations were being conducted with a view to throwing more precise light upon the exact condition, chemical and physical, of these substances in the sun. It could not at present be decided whether many of the substances we call elementary were set at liberty by the very high temperature of the sun. Another circumstance of great importance was the probable pressure of the gases at different portions of the sun's surface. This was also connected with another problem of great interest which had still to be worked out, namely, the heat of different portions of the sun.

When an eclipse occurred we were then taught that what we called the sun was a comparatively small part of that great luminary. So far as extent went, really we only saw the smallest part of the sun. There was more of the sun invisible than was visible to us Dr. Huggins next pointed to a diagram which he observed was, in his opinion, one of the most beautiful that had been obtained of the appearances which were round the sun at the time of a total solar eclipse. The great black mass visible to the audience was the moon, which was slightly larger than that part of the sun which was visible to us, and covered it entirely, and then there came into view at the edges of the moon's disk the rays of coronal light and grand masses of red fire. The reason why the sun's corona and red flames did not appear to us was not because of their brightness, but because of the imperfect transparency of our atmosphere. Diagrams were also shown illustrating the lambent flames visible at the edge of the sun's disk during an eclipse, and describing the method by which these objects could be seen without an eclipse. In concluding Dr. Huggins said: Of course it was obviously quite impossible for me, in the course of one hour, to give you a finished picture of this subject.

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